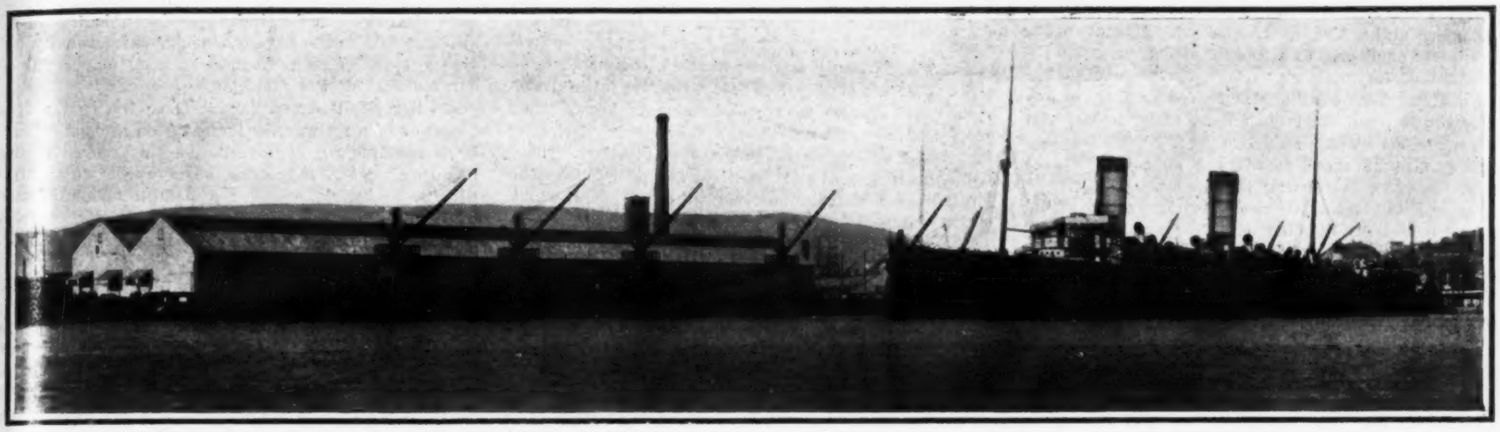
SCIENTIFIC AMERICAN SUPPLEMENT. No 1787

Entered at the Post Office of New York, N. Y., as Second Class Matter.
Published weekly by Munn & Co., Inc., at 361 Broadway, New York.
Charles Allen Munn, President, 361 Broadway, New York.
Frederick Converse Beach, Sec'y and Treas., 361 Broadway, New York.

Scientific American, established 1845.
Scientific American Supplement, Vol. LXIX., No. 1787.

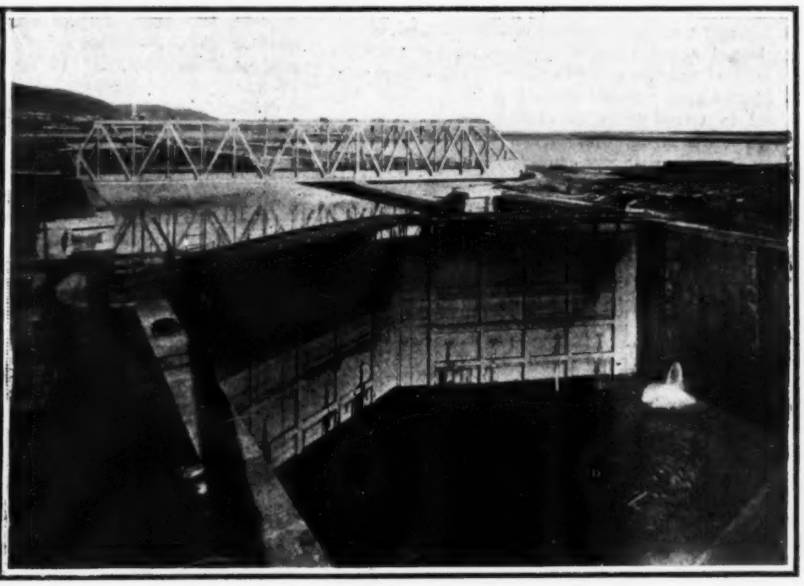
NEW YORK, APRIL 2, 1910.

Scientific American Supplement, $5 a year.
Scientific American and Supplement, $7 a year.

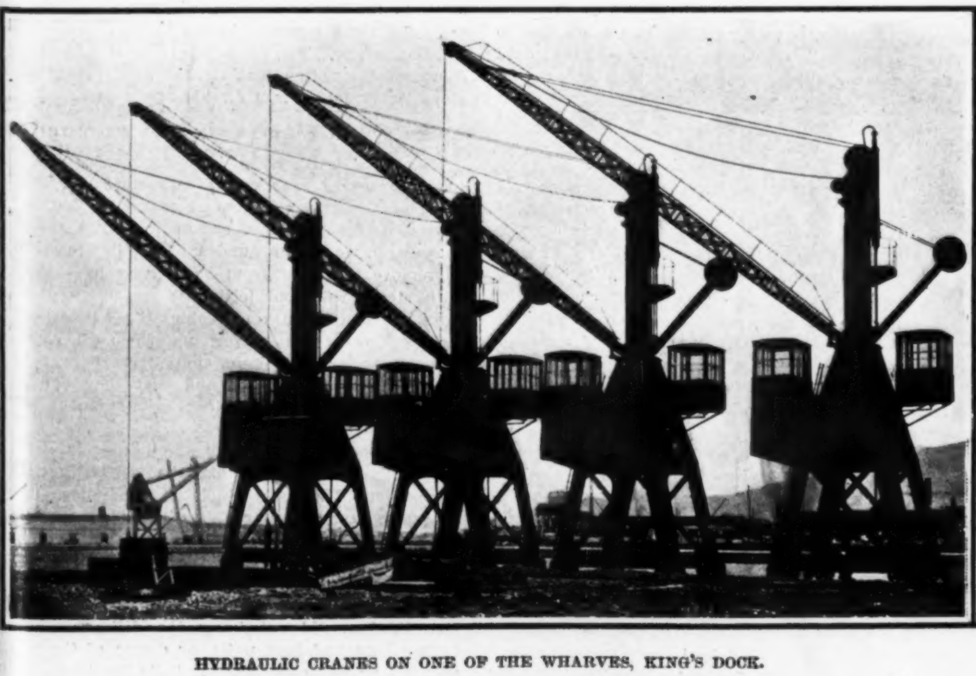


R. M. S. "LUCANIA" IN BERTH AT WEST WHARF OF KING'S DOCK, SWANSEA.

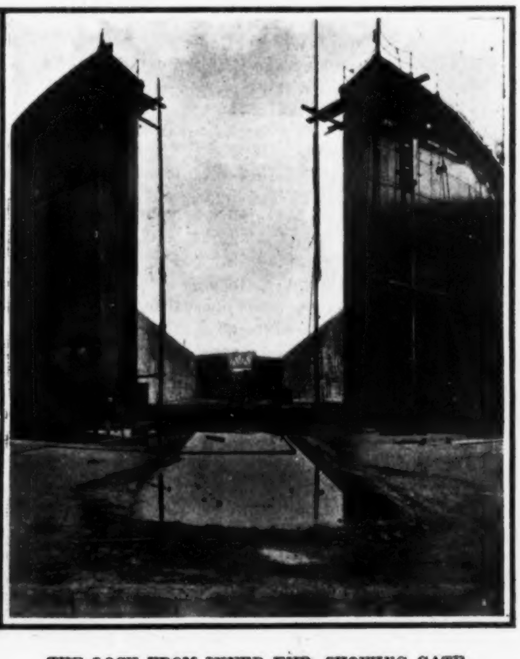
A TITAN CRANE PLACING LARGE STONE ON OUTSIDE OF SEA EMBANKMENT.



LOCK FROM OUTER END. FORTY FEET OF WATER ON OUTER CELL AT HIGH WATER OF SPRING TIDE.



HYDRAULIC CRANES ON ONE OF THE WHARVES, KING'S DOCK.



THE LOCK FROM INNER END, SHOWING GATE.

THE NEW KING'S DOCK AT SWANSEA.—[See page 216.]

POLICE DOGS.

CANINE DETECTIVES.

BY KARLERNST KNATZ.

Good detectives are very rare, and correspondingly expensive. Hence a police administration conducted with due regard to economy could scarcely afford to employ a veritable Sherlock Holmes, if he could be found. Ordinary detectives and policemen, on the other hand, cannot cope successfully with the highly developed and refined methods of the modern criminal. The police authorities, confronted with this dilemma, have lately employed, to a limited extent, an auxiliary possessed of instincts which take the place of detective genius. This auxiliary is the police dog, which, in Berlin at least, is giving almost daily proof of its fitness for detective and police work. The training and practice of these four-legged guardians of the peace are most interesting to witness.

One morning last summer, the writer was conducted by Police Major Klein, the organizer and commander of the canine police force of Berlin, to a public garden in the suburbs. Here we found assembled a number of the men of the corps, all selected from the general body of police with especial reference to their liking for dogs. On the grass lay the dogs attached to the northwestern police station—gaunt, sinewy animals with shrewd faces, of the German sheep dog breed. We took seats and the drill commenced. The dogs were required to give tongue at a signal and to lie still at assigned stations until the trainer, moving away from them, summoned them by a call or a whistle. Even when running at full speed they instantly stopped and lay down at the word of command. All of the dogs leaped a seven-foot fence with ease, even when they carried heavy objects in their mouths. The next test of obedience was the refusal of food. A dog was chained to a tree and ordered to guard some object, such as a bunch of keys. When I cautiously fished for the treasure with my cane, the dog rushed at me savagely, tugging at his chain, growling and gnashing his teeth. An appetizing piece of sausage that I had offered to distract the animal's attention, was regretfully sniffed at and then contemptuously ignored. The experiment was repeated with all the dogs, with the same result. Frack, the champion of the corps and the winner of many prizes, did not even turn his nose toward the sausage.

One of the police officers then beckoned to Grete, a bitch of remarkable beauty, and directed her, by a gesture, to search for something hidden in a clump of bushes at the back of the garden. Grete followed the indicated direction and soon returned with a two-mark piece in her mouth.

After witnessing these specimens of school work, we went out into the open fields, to see the dogs track and arrest supposed criminals. A crowd of curious onlookers had already collected. A young man, who subsequently proved an uncommonly tame and stupid malefactor, was enveloped in a heavy padded leather coat, which showed the marks of numerous dog bites, and directed to lay a trail by walking across a meadow, and then to conceal himself. Several other trails had been previously laid across the same field. A striking exhibition was then witnessed. Frack and another dog followed all of the trails, bringing back a handkerchief from one of them, and finally traced and discovered the criminal and sprang upon him, but released him instantly at their master's command. The sight made me shudder. These wise and faithful animals could become savage and ravenous beasts when duty demanded. They always seize their victim by the throat or the wrist, and a wild pistol shot only redoubles their zeal and ferocity. The poor fellow who impersonated the criminal was bathed in sweat, which was not due solely to his leather coat.

The exhibition closed with an aquatic performance in the neighboring canal. Here the leading part was taken by a dog which has actually rescued a number of persons from drowning and has brought many floating corpses to land. A puppet filled with cork was thrown into the canal. The dog swam out to it, seized it by one arm and drew it to shore, displaying astonishing skill, care and strength. In the water, as on land, the dogs surprised me by the promptitude and precision with which they advanced, retreated and turned to right or left, at the word of command.

The canine police corps of Berlin, already very efficient, is steadily improving, under the able guidance of Major Klein, who has given me his views on the functions and the training of police dogs in a personal letter, from which I quote the following:

"The fidelity and affection of the dog must be won, maintained and rewarded by kind and intelligent treatment. All dogs, even those of naturally obstinate disposition, are amenable to kindness. Many dogs are so sensitive that they cannot endure even a scolding, while a rash blow or violent imprecation may spoil them for a long period, if not forever. Obstinate and unimpressionable dogs may be treated with greater severity, after the trainer has clearly recognized their peculiarities. Such dogs must be taught that their willfulness and obstinacy are useless, and that man is their master, and the whip, applied at the right moment, is an efficient aid in inculcating this lesson."

In an article on police dogs, recently written by Dr. Th. Zell, this keen observer of animals gives a scientific explanation of the remarkable faculty of tracing persons and things by scent, upon which the efficiency of the dog, as an ally in the war against criminals, chiefly depends. Dr. Zell illustrates this faculty by comparing its absence with the defect of vision known as color blindness. A person who is color blind may see as clearly and sharply as a person with normal vision, but he cannot gather strawberries with conspicuous success, because the ripe and the green berries and the green leaves appear to him alike in color. A similar defect is exhibited by an ear that is unmusical, or tone deaf. The musical person distinguishes small differences in pitch and quality, which have no existence for the person who is tone deaf, although his acuteness of hearing, as shown by the ability to perceive very faint sounds, may equal or surpass that of the musician.

A color blind person cannot trace the course of a vessel or of a railway, as indicated on a chart or map by a red line, which is followed easily by a person of normal sight. If the dog, as we appear to be justified in assuming, attaches a definite mental image to each distinct odor, the animal's ability to trace the right trail through a labyrinth of other trails becomes explicable.

The following hints are abstracted from Major Klein's manual for the police dog service:

"The dog must be at once the assistant and the protector of the police officer, a trustworthy and respect-inspiring companion, giving to the solitary patrolman a sense of security and a moral support. He must be attached to his master alone, regarding other persons with suspicion and not allowing them to approach him. He should precede the patrolman to spy out the land. The keen scent, watchfulness and agility of the dog should facilitate the discovery of suspicious persons and conditions and save the policemen many useless steps.

"In the arrest and conveyance of prisoners, and in resisting attempts to free prisoners, and attacks upon officers, the dog should afford very valuable assistance, while in the pursuit of a criminal detected in the act of crime, the dog should, so to speak, extend the reach of the policeman's arm, by 'dogging' the footsteps of the fugitive, following his fresh tracks and finally holding him until the policeman arrives. The police dogs are also expected to assist in the rescue of persons in danger of drowning, and to follow persons whose manner indicates that they are contemplating suicide, and give warning by 'baying.' In raids the dogs should hunt out concealed persons and betray them to the police."

In Paris, according to the newspapers, the police dog has already made his appearance on the theatrical stage, thus following in the footsteps of Sherlock Holmes, Lecoq and other real and fictitious human prototypes. This could never have occurred in a theater managed by Goethe, who left the theater in a rage when a dog was brought upon the stage.—Translated for the Scientific American Supplement from Ueber Land und Meer.

QUANTITATIVE VS. QUALITATIVE THINKING.*

By R. W. Raymond.

It was not so very long ago that the catalogue of a certain college announced the Rev. ————, D.D., as president, and professor of ancient and modern languages and history; of mental, moral and natural philosophy and applied science! A small and in some respects a ludicrous beginning—but it contained at least the aspiration, which was a prophecy, of the manifold and multiform training with which our best universities are now equipped and competent to furnish.

In the second place, this gathering emphasizes the underlying unity of all these specialties: Mining, metallurgical, civil, mechanical, electrical, chemical engineers—you are all engineers. Even the architects are engineers or else they are not fully accomplished as architects. You must, methinks, all possess something in common which makes you engineers, and which, as engineers, you are bound to make effective for the benefit of your generation.

I am not going to excite your ridicule or aid the somnolency of your digestion of such a feast as this, by offering you a complete, inclusive and exclusive definition of an engineer. Certainly, it means more than simply a man who runs an engine. Somewhere in the '70s of the last century, when the American Institute of Mining Engineers held its first meeting in Baltimore, a leading manufacturer of that city said to one of us, "If you have any influence with your brethren in that engineers' convention, I hope you will urge them not to order a strike. In these times, $2.50 a day is really all that we can afford to pay our engineers." Of course, he was told that ours was not a labor union, but a union of laborers, who never struck for high wages, but simply earned them and got them; and that our chief purpose was not to increase the wages, but to improve the work.

QUANTITATIVE THINKING ESSENTIAL.

While I do not propose to define the engineer, I beg to suggest one element which characterizes, I think, all true engineers. And I will use as a name for it a term borrowed from the laboratory. The engineer's thought and work are quantitative, not merely qualitative. Engineering lies at the base of the edifice of civilization; but the progress of civilization is often erroneously measured by mere inventions. Now inventions may sometimes be made and must always be perfected and utilized by engineers. But inventors who are nothing more fare badly, as they ought to.

How often we hear the accusation that the world has ill repaid the first propounder of an idea which in the hands of another has proved greatly profitable. But, in truth, it is not the happy thought about a thing, but the practical doing of the thing, that deserves reward. No doubt the first man, looking at the first bird, wanted to fly, and tried to fly, but the trouble was he couldn't fly. Should we now hunt up his heirs and pay them for the invention of their ancestor? His work was merely qualitative.

Once upon a time a pale, inspired inventor climbed up to my office and asked me to help him to go to Colorado. He had invented the idea of using electricity in mining and metallurgy; and he wanted to go to the Rocky Mountains, because at their summits electricity was so abundant. His notion was a qualitative one. Yet, no doubt, if he is still alive, he cherishes the notion that he has been robbed of the glory justly due to him for his bright idea.

INEFFICIENCY OF THE MERELY QUALITATIVE THINKER.

These qualitative enthusiasts grow more and more numerous every year, with the increase in half-educated practitioners of "applied science." And the old errors continuously crop out anew. After every big snowstorm, we have a number of qualitative proposals to remove the snow from our streets by thawing it with steam jets, and letting it run away of itself. What is still more remarkable, the editors of our leading newspapers gravely publish such communications, under the impression that there may be something in the notion, and that they had better not disparage it lest haply they be found fighting against science.

After the great blizzard a storekeeper in Brooklyn undertook to apply quantitatively this qualitative proposition. Having a 16-foot heap of snow opposite his store, and a cellar full of old boxes under it, he dug in the drift a chamber with a chimney, crammed this stove with the kindling wood from the cellar, lit the fire, and waited for the mountain to disappear in fervent heat. He, being a qualitative thinker, did not know the difference between temperature and heat units, or dream of the amount of work he was expecting his little fire to do. When he had burned all his wood he abandoned his faith in science.

* Excerpts of an address given at the dinner of alumni of the Schools of Science and the School of Architecture of Columbia University. Republished here from the Engineering and Mining Journal.

I could multiply such illustrations indefinitely. But I may be permitted, in conclusion, to say generally and inoffensively, that qualitative thinking is one of the dangers of our day. Great popular manias, fads of special reform, exposures of public evils or hardships, appeals for governmental interference, all are, or are likely to be, merely qualitative. I think it was Thomas Jefferson who said, in substance, "A free people never loses its liberties by surrender to force. It gives them away in moments of enthusiasm."

It is the duty of engineers, who have been trained to regard and handle things quantitatively, to contribute this needed element to the thought and action of their time. Let them make no haste to conclusions; let them count the cost, measure the means, weigh the sacrifices, appraise the gains and the losses—in a word, hold fast to the distinction between heat units and mere degrees of temperature.

THE "TYPICAL" MAN WHO DOES NOT EXIST.

THE COMPOSITE OF A HUMAN GROUP.

BY PROF. R. S. WOODWORTH

One of the most agreeable and satisfying experiences afforded by intellectual pursuits comes from the discovery of a clean-cut distinction between things which are superficially much alike. The esthetic value of such distinctions may even outweigh their intellectual value and lead to sharp lines and antitheses where the only difference that exists is one of degree. A favorite opportunity for this form of intellectual exercise and indulgence is afforded by the observation of groups of men. The type of man composing each group—that is what we should like to find; and we hear much of the "typical" scientist, the typical business man, the typical Englishman or Frenchman, the typical southerner, the typical Bostonian. The type of any group stands as a sort of ideal within the group, and, more or less caricatured, as the butt of the wit of other groups. There is one peculiar fact about these types: you may have to search long for an individual who can be taken as a fair example. And when you have at last found the typical individual, you may be led to ask by what right he stands as the type of the group, if he is a rarity amid it.

If we would scientifically determine the facts regarding a group of men, we should, no doubt, proceed to examine all the individuals in the group, or at least a fair and honest representation of them. The first fact that meets us when we proceed in this way is that the individuals differ from one another, so that no one can really be selected as representing the whole number. We do find, indeed, when we measure the stature of any other bodily fact, or when we test any native mental capacity, that the members of a natural group are disposed about an average, many of them lying near the average, and few lying far above or far below it; and we thus have the average as a scientific fact regarding the group. But the average does not generally coincide with the type, as previously conceived, nor do the averages of different groups differ so much as the so-called types differ. Moreover, the average is itself very inadequate, since it does not indicate the amount of variation that exists within the group—and this is one of the most important facts to be borne in mind in understanding any collection of individuals. It is specially important in comparing different groups of men, since the range of variation within either group is usually much greater than the difference between the averages of the groups. The groups overlap to such an extent that the majority of the individuals composing either group might perfectly well belong to the other.

No doubt statements like this will be readily accepted as far as concerns the different nations belonging to the same race. One could not seriously doubt that the nations of Europe, though they might differ slightly on the average, would so much overlap one another that, except for language and superficial mannerisms, the great majority of the members of one nation might be exchanged with a majority from another nation without altering the characteristics of either. But when we extend our view to all the peoples of the earth, the case would at first appear quite changed. Certainly whites and negroes do not overlap, to any extent, in color of skin, nor negroes and Chinamen in kinkiness of hair, nor Indians and Pygmies in stature. Such specialization of traits is, however, the exception. Whites and negroes, though differing markedly in complexion and hair, overlap very extensively in almost every other trait, as, for example, in stature. Even in brain weight, which would seem a trait of great importance in relation to intelligence and civilization, the overlapping is much more impressive than the difference; since while the brain of negroes averages perhaps two ounces lighter than the brain of Europeans, the range of variation within either race amounts to 25 ounces.

Our inveterate liking for types and sharp distinctions is apt to stay with us even after we have become scientific, and vitiate our use of statistics to such an extent that the average becomes a stumbling-block rather than an aid to knowledge. We desire, for example, to compare the brain weights of whites and of negroes. We weigh the brains of a sufficient number of each race—or let us at least assume the number to be sufficient. When our measurements are obtained and spread before us, they convey to the unaided eye no clear idea of a racial difference, so much do they overlap. If they should become jumbled together, we should never be able to separate the negroes from the whites by aid of brain weight. But now we cast up the average of each group, and find them to differ; and though the difference is small, we straightway seize on it as the important result, and announce that the negro has a smaller brain than the white. We go a step further, and class the white as a large-brained race, the negro as a small-brained. Such transforming of differences of degree into differences of kind, and making antitheses between overlapping groups, partakes not a little of the ludicrous.

We seem to be confronted by a dilemma; for the group as a whole is too unwieldy to grasp, while the average, though convenient, is treacherous. What we should like is some picture or measure of the distribution of a given trait throughout the members of a group; and, fortunately, such measures and pictures can be had. Convenient and compact measures of variability are afforded by the science of statistics, and are of no less importance than the average. But still better, because closer to the actual facts, are graphic or tabular pictures of the distribution of the trait, showing the frequency with which it occurs in each degree. The distribution of a trait is for some purposes more important than the average. Let us suppose, for instance, that two groups were the same in their average mental ability, but that one group showed little variation, all of its members being much alike and of nearly the average intelligence, while the other group showed great variability, ranging between the extremes of idiocy and genius. It is evident that the two groups, though equal on the average, would be very unequal in dealing with a situation which demanded great mental ability. One master mind could supply ideas for the guidance of the group, and his value would far outweigh the load of simpletons which the group must carry.

If groups of men differ in average intelligence, this difference would have an influence on their effectiveness in mental work, and so, no doubt, on their advance in civilization. If groups differ in variability, this would probably have a still greater influence. There is one respect in which groups certainly do differ. They differ in size, and size is an important consideration, even from a purely biological point of view. The more numerous the individuals born into a group, the greater the absolute number of gifted individuals to be expected; and in some respects it is the absolute rather than the relative number of able men that counts. Besides this, the larger the group, the greater the chance of its producing a truly effective genius, just as, in the experiments of Burbank and other breeders, a vast number of plants are grown, in order to increase the chance of sports occurring.

One further consideration of this partly biological, partly statistical nature should be brought forward. When the individuals composing a group are measured or tested in several traits, it is found that those who rank high in one trait do not always rank high in others. On the whole, there is more correspondence than opposition; an individual who ranks well in one trait is rather apt to rank well in others. The correlation, as we say, is positive, but it is far from perfect. The individuals most gifted with ability in war are not altogether the same individuals who are ablest in government, or in art or literature, or in mechanical invention. This fact is not only of importance in reaching a just conception of a group, but it should be considered in comparing different groups. The circumstances surrounding a group call for certain special abilities, and bring to the fore the individuals possessing these abilities, leaving in comparative obscurity those gifted in other directions. Judging the group largely by its prominent individuals, we get the impression that the group is gifted in certain lines and deficient in others. A nation whose circumstances call for industrial expansion and the exploitation of natural resources gives prominence to those of its members who are successful in these pursuits, and leaves in obscurity many who have native capacity for military leadership. Should war come to such a community, time and bitter experience are often necessary before the leadership can be transferred from the previously eminent men to those obscure and often despised individuals who are capable of doing best service in the new direction. This lack of perfect correlation between various abilities makes it difficult to judge of the capacity of a group of men by casual observation; and we must accordingly discount largely the appearance of specialization of mental traits in different peoples.

* Abstracted from the address of the vice-president and chairman of Section H, Anthropology and Psychology, of the American Association for the Advancement of Science.

A SCIENTIFIC CLASSIFICATION OF SMELLS.

In a recent number of the Lancet a correspondent of that journal makes a plea for a scientific classification of smells, just as there is a classification of colors and of sounds.

"The sense of smell," writes he, "is the function of the olfactory nerve, which also distinguishes the various flavors, and this may be the reason why so many persons cannot distinguish smell from taste. The four primary tastes are sweet, bitter, acid, and salt, and these have no effect on the olfactory nerve; hence analysis of this nerve does not delete the sweet, bitter, acid, or salty character of substance, but does of the flavoring agents. But paralysis of the glosso-pharyngeal and certain fibers of the fifth nerve destroys the sense of taste, but not flavor. Some of the senses are very fully classified, vision and hearing in particular; probably these give the greater pleasure to the great majority. We have primary and secondary colors, and therefore to describe a color as horrid would not be acceptable, although such passes muster for the smell."

How is a basis of a classification to be arrived at? Is such a classification, in truth, possible? The writer thinks that it is, and he makes the following suggestions: "In the selection of odoriferous substances one recognizes the importance of choosing pure definite chemical bodies so as to avoid mixed smells, and that they should be capable of ready dilution, as strong odors not only quickly temporarily paralyze the olfactory nerve endings, but also irritate the fifth nerve. The concocted names of the primary odors should be easily adapted to affixes and prefixes. Having adopted the standard of odors and their names, I would suggest sets of them at the elementary schools to familiarize the children with them and their names, as is done at present in the case of colors. The foundation laid of an octave of smells, it would not be so difficult as hitherto to impart more definitely and intelligently the character of a smell to others in legal and medical cases. As a preliminary classification of the primary odors we might select such bodies as 1, ammonia; 2, sulphureted hydrogen; 3, bromine; 4, valerianic acid; 5, ether; 6, menthol; 7, camphor; 8, artificial musk; 9, nitro-benzol. Next we have to concoct a name for each of these odors apart from the names of the substances, such as is done with colors. For example, red does not pertain to any particular substance, but is the quality of large numbers, and we can qualify the quality in various ways, such as reddish-yellow and reddish-brown, or yellowish-red and brownish-red. After settling the primary odors and names we could, I think, in the same manner describe the odors fairly well of a larger number of substances."

Creosoted piles driven in sea water in a dike at New Haven, Conn., about 1885, were examined a year ago. About 5 per cent. of the piles in the channel arm and 25 per cent. in the shore arm were in bad condition, due to rotting, beginning at the top in the center. The engineer suggests that deterioration might not have occurred if the tops had been protected.

ELECTRICITY AND LIGHT IN MODERN MEDICINE.

A NEW THERAPEUTIC AGENT.

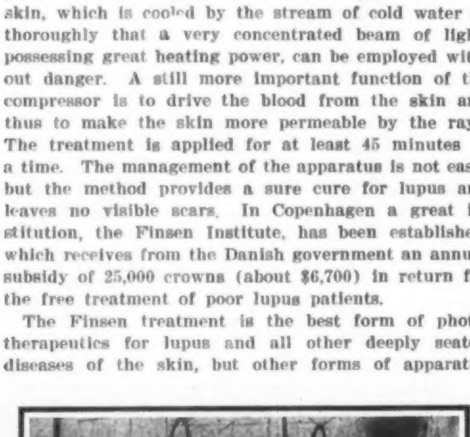
BY DR. THEODOR SCHUELER.

A PROMINENT place among the physical agents employed in modern methods of therapeutics has been occupied for the past decade by light, and especially by electric light, which is used chiefly in the form of "light baths" and is furnished by arc and incandescent lamps, separately or in combination. As a rule, however, the electric light bath may be regarded merely as an agreeable and elegant variety of hot bath, and a substitute for the Roman, Russian or hot-air baths, and the Turkish or steam bath. No peculiar therapeutic properties are now attributed to the rays of the electric arc or incandescent lamp.

The system of light baths which was invented by the late Prof. Niels R. Finsen of Copenhagen, stands on a somewhat different footing and forms the basis of modern photo-therapeutics and the inspiration of all subsequently devised photo-therapeutic apparatus. The efficiency of the treatment of bacterial diseases of the skin, especially lupus vulgaris, or tuberculosis of the skin, with concentrated ultraviolet radiations, is now generally recognized. Finsen proved that this efficacy is due chiefly to two properties of the violet and ultraviolet rays; their power to penetrate the skin and their destructive action on bacteria.

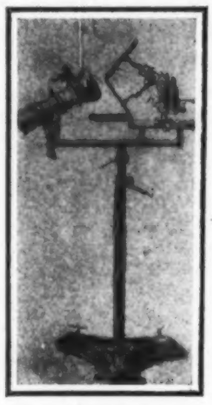
In Finsen's apparatus, the radiation from an arc lamp is filtered through water, which absorbs most of the ultrared, or "heat," rays, and is concentrated upon the skin by lenses of quartz, which does not, like glass, absorb much of the ultraviolet radiation and which aids in absorbing the ultrared rays. The apparatus is made in two forms, a large model for use in hospitals and a smaller one, known as the Finsen Rheyn lamp, for the use of physicians in private practice. The latter consists of a 20-ampere direct-current arc lamp, a "concentrator" and a "compressor." The apparatus is so mounted on an iron stand that it can be turned in any direction. The concentrator is a wide metal tube, containing a number of quartz lenses, through which a current of cold water flows continuously. The compressor is attached to the end of the concentrator and consists of a short brass cylinder, closed at each end with a plate of quartz and filled with the circulating cold water. The terminal quartz plate is pressed against the diseased portion of the skin, which is cooled by the stream of cold water so thoroughly that a very concentrated beam of light, possessing great heating power, can be employed without danger. A still more important function of the compressor is to drive the blood from the skin and thus to make the skin more permeable by the rays. The treatment is applied for at least 45 minutes at a time. The management of the apparatus is not easy, but the method provides a sure cure for lupus and leaves no visible scars. In Copenhagen a great institution, the Finsen Institute, has been established, which receives from the Danish government an annual subsidy of 25,000 crowns (about $6,700) in return for the free treatment of poor lupus patients.

The Finsen treatment is the best form of photo-therapeutics for lupus and all other deeply seated diseases of the skin, but other forms of apparatus

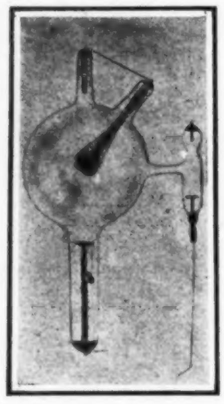


THE DESSAUER APPARATUS FOR APPLYING ROENTGEN RAYS.

have been devised for the treatment of eczema, baldness, erysipelas and other diseases of superficial character. Within the last few years many arc lamps with metallic electrodes have been invented, which emit a far larger proportion of violet and ultraviolet rays than the ordinary carbon arc lamp produces. The largest proportion of these rays is emitted by the mercury vapor lamp invented by Cooper Hewitt, which is used largely in photography. Dr. Knech substituted for the glass tube of the Cooper Hewitt lamp a tube of quartz, and thus greatly increased the emission of violet and ultraviolet rays, which, as was observed above, are absorbed very rapidly by glass,

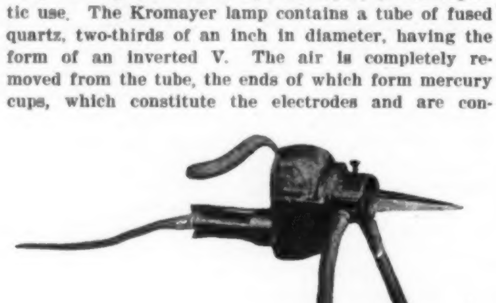


FINSEN-RHEYN LAMP.



A ROENTGEN TUBE.

but very little by quartz. The quartz mercury vapor lamp was again modified by Kromayer, for therapeutic use. The Kromayer lamp contains a tube of fused quartz, two-thirds of an inch in diameter, having the form of an inverted V. The air is completely removed from the tube, the ends of which form mercury cups, which constitute the electrodes and are con-



KROMAYER LAMP WITH QUARTZ EXTENSION.

nected to the lighting wires. This luminous tube is inclosed in a globe, which is also made of fused quartz, and the globe is inclosed in a water-tight case of nickeled brass, provided with a quartz window, which can be pressed directly against the skin of the patient. The space between the quartz globe and its metal case is filled with cold water, which enters continuously through a tube at the bottom of the case and flows out through a tube at the top. The lamp is ignited by tipping it and thus causing a thin stream of mercury to flow along the tube and establish connection between the electrodes. When the lamp is righted and the thread of mercury is broken a powerful luminous arc is formed in the mercury vapor which fills the tube. Local treatment, with or without pressure, can be applied very conveniently with this lamp, but great care must be taken to avoid burning the skin. This treatment is efficacious in all forms of eczema, carbuncles and ulcers of the legs and various parts of the skin and mucous membranes, but especially in "wet" eczema and in circular baldness. Treatment with pressure is employed especially in lupus and burns. In the opinion of the writer and other physicians experienced in photo-therapeutics, however, the quartz window of the Kromayer lamp should not be pressed directly upon the skin, for various reasons, and especially because the window cannot be thoroughly sterilized after each application. I have, therefore, attached to the window a water-cooled metal tube, with which are fitted blocks and closed tubes of quartz, of the sizes and shapes most suitable for application to the external skin and the mucous membranes which line the various cavities of the body.

Another new and very valuable therapeutic agent is furnished by the Roentgen rays, which have exerted a powerful influence on every branch of medicine. For the production of these rays electric currents of high tension and small quantity, or strength, are required. The reader who is not well versed in electrical theory may obtain an idea of the meaning of these terms (pretty nearly as good an idea as the electrician possesses, by the way) by picturing the electric current as a current of water flowing through a pipe. The electric tension, or electromotive force, corresponds to the pressure, or "head," of the water, and the strength of the current, in each case, is the quantity of water, or of electricity, that passes in a given time. The unit of tension is called a volt, and the unit of current strength is called an ampere. When an electric current of very high voltage, such as is produced by an electrical machine or a Ruhmkorff induction coil, is interrupted about 2,000 times a minute by suitable mechanism, and the interrupted current is caused to flow from one metallic electrode to another in a so-called "vacuum tube" (which is really a glass tube from which nearly, but not quite, all the air has been pumped), that part of the glass wall which is directly opposite the negative electrode or cathode (which is flat or slightly concave) glows with a greenish light and emits, outside the tube, the invisible rays which are known as Röentgen, or X rays. These rays possess a remarkable power of penetrating all substances to a degree dependent on the density of the substance. They also affect photographic plates and cause fluorescent substances to glow, and they produce a peculiar stimulation when they fall on the skin.

If the hand is placed on a photographic plate, which is inclosed in a plate-holder, and Roentgen rays are thrown on it from above, a photograph showing every bone of the hand is produced in a few seconds. It is even possible to produce an X-ray photograph of the ribs and the lungs in a single second. In this way diagnosis can often be greatly facilitated. A bullet, a needle or the fracture of a bone can be quickly and accurately located, tuberculous spots in the lungs can be discovered and, with especially good apparatus, the presence of a stone in the kidney can be detected. For all of these purposes "hard" Roentgen tubes are employed, i. e., tubes containing only an infinitesimal quantity of air, or a very high vacuum. "Soft" tubes, which contain a little more air, are used in the application of Roentgen rays to the skin for the cure of disease. The rays destroy diseased cells and hence, if they are applied promptly and persistently, they remove and heal morbid growths of various kinds. If the rays produced by soft Roentgen tubes are applied for a very long time their destructive action extends to the healthy tissue. Hence, soft tubes can be used with advantage only in the treatment of superficial growths and diseases, such as the various forms of acute and chronic eczema, psoriasis, scaldhead, warts, etc. I never apply soft-tube rays longer than ten minutes or oftener than three times a week, and I have never observed any injurious effect in the ten years during which I have employed Roentgen rays. Sometimes, in spite of every precaution, the hair of a small part of the head is destroyed, but a new and much more luxuriant growth always appears within a few months.

Quite recently, "hard" Roentgen tubes have been employed in the treatment of deep-seated affections of the skin, without injuring the surrounding healthy

QUARTZ BLOCKS AND TUBES FOR APPLYING ULTRAVIOLET RAYS, WITH PRESSURE, TO VARIOUS PARTS OF THE BODY.

tissue, even when the application is continued for an hour or longer. The method of using hard tubes was devised by Prof. Poelhes, and has been improved by Dessauer, who employs very "hard" tubes, in which the vacuum is so high that an electromotive force of 120,000 volts is required to force a current through

them. This high voltage is produced by specially designed induction coils, which are immersed in oil in order to secure perfect insulation. The coil is operated by an alternating current, in connection with a Goetze rectifier and a Wehnelt interrupter. The induced current passes through two or more extremely "hard" Roentgen tubes, which are placed from 20 to 40 inches distant from the patient, who is protected from injurious radiations by the interposition of a large pane of window glass. The rays can be applied in this manner for two or three hours, without producing any injury to the healthy tissue. Very gratifying results have been obtained with Dessauer's apparatus. I employ this method in the treatment of sarcoma (malignant tumors), cancerous growths in their earliest stages, goitre, Basedow's disease, tumors of the lymph glands and pernicious anemia. True cancer, if well developed, requires the knife, but after the operation I apply the Roentgen rays to the wound and the entire body, in order to destroy lurking germs and prevent a relapse.—Die Gartenlaube.

SOLAR VORTICES AND MAGNETIC FIELDS.—I.*

RECENT STUDY OF THE SUN.

BY PROF. GEORGE E. HALE.

THE spectroheliograph, which tells us of the existence of solar vortices, is a natural outcome of the application of the spectroscope in astronomy, where Englishmen were foremost among the pioneers. The detection of a magnetic field within these vortices followed directly from Zeeman's beautiful discovery of the influence of magnetism on radiation—a logical extension of the earlier work of Faraday—and from the classic investigations of Crookes and Thomson on the nature of electricity.

It is customary to distinguish sharply between the observational and experimental sciences, including astronomy in the former. In physics or chemistry the investigator has the immense advantage of being able to control the conditions under which his observations are made. The astronomer, on the other hand, must be content to observe the phenomena presented to him by the heavenly bodies, and interpret them as best he may. I wish to emphasize the fact, however, that the distinction between these two methods of research is not so fundamental as it may at first sight appear. In 1860 a laboratory, in which experiments were conducted for the interpretation of astronomical observations, was established by Sir William Huggins on Upper Tulse Hill. The advantage of imitating celestial phenomena under laboratory conditions was thus appreciated half a century ago. I shall indicate later how important a part such a laboratory plays in the work of the Mount Wilson Solar Observatory. I shall also show that in other ways the astronomer may advantageously follow the physicist, particularly in the choice of observational methods and in the design of instruments of research.

Sun-spots were discovered as soon as Galileo and his contemporaries directed their little telescopes to the sun. In fact, ancient Chinese records indicate that spots of exceptional size had been detected by the naked eye many centuries before. Long after their discovery, the most diverse views were held as to the nature of sun-spots. Sir William Herschel mentioned the uncertainty which had existed prior to his time, remarking that the spots had been variously described as solid bodies revolving about the sun, very near its surface; the smoke of volcanoes; smoke floating on a liquid surface; clouds in the solar atmosphere; the summits of solar mountains, uncovered from time to time by the ebb and flow of a fiery liquid, etc. In Herschel's own view the spots are to be considered as the opaque body of the sun seen through openings in the luminous atmosphere which envelops it. Indeed,

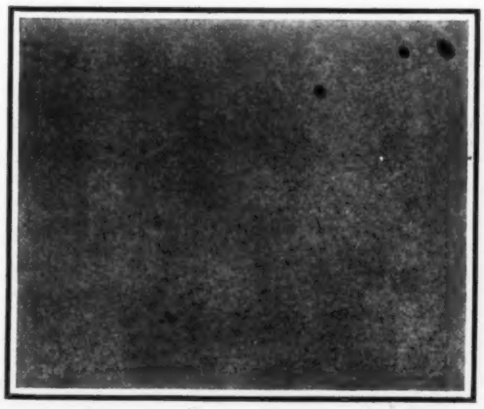


FIG. 1.—DIRECT PHOTOGRAPH OF SUN-SPOT GROUP.

1908, April 30, 6h 25m A. M. Pacific standard time.

he considered that the sun should be regarded as the primary planet of our system, and even suggested the probability that it is inhabited. "Whatever fanciful poets might say, in making the sun the abode of blessed spirits, or angry moralists devise, in pointing it out as a fit place for the punishment of the wicked, it does not appear that they had any other foundation for their assertions than mere opinion and vague surmise; but now I think myself authorized, upon astronomical principles, to propose the sun as an inhabitable world, and am persuaded that the foregoing ob-

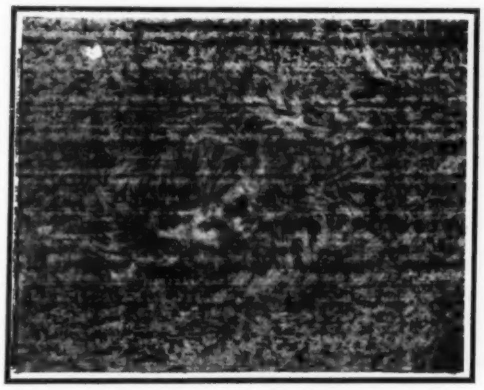


FIG. 3.—SAME REGION OF THE SUN, SHOWING THE HYDROGEN ($H\alpha$) FLOCCULI.

1908, April 30, 5h 06m P. M. Pacific standard time.

servations, with the conclusions I have drawn from them, are fully sufficient to answer every objection that may be made against it."*.

Sir John Herschel did not abandon the idea of an opaque solar globe, but suggested that hurricanes or tornadoes might account for the piercing of the two strata of luminous matter which ordinarily conceal this globe. "Such processes cannot be unaccompanied by vorticose motions, which, left to themselves, die away by degrees and dissipate—with this peculiarity, that their lower portions come to rest more speedily than their upper, by reason of the greater resistance below, as well as the remoteness from the point of action, which lies in a higher region, so that their center (as seen in our water-spouts, which are nothing but small tornadoes) appears to retreat upward. Now, this agrees perfectly with that which is observed during the obliteration of the solar spots, which appear as if filled in by the collapse of their sides, the penumbra closing in upon the spot, and disappearing after it."

We now know that sun-spots are brighter than the

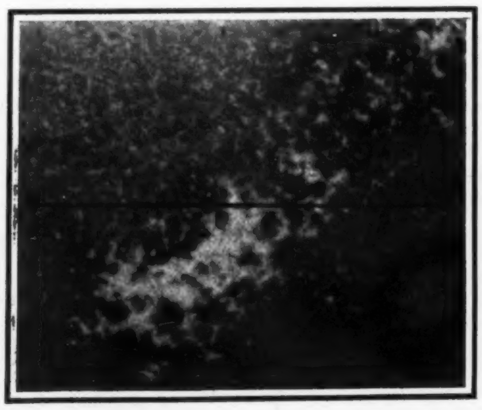


FIG. 2.—SAME REGION OF THE SUN, SHOWING THE CALCIUM (H_1) FLOCCULI.

1908, April 30, 4h 43m P. M. Pacific standard time.

brightest arc light, and that their apparent darkness is merely the result of the contrast with the intensely brilliant surface of the photosphere. We also know that the sun is a gaseous globe, attaining a temperature of about 6000 deg. at its surface, and perhaps millions of degrees at its center. If we examine a large-scale photograph of a sun-spot we see that it consists of a dark central region, called the umbra, and a surrounding area, decidedly less dark, called the penumbra. The structure of a spot, as this admirable photograph by Janssen shows, is granular, like that of the photosphere. In the penumbra these granulations seem to group themselves more or less radially, as though under the influence of some force directed toward or away from the umbra. Unfortunately, direct photographs of the sun have not yet attained such perfection as to show the most minute details of sun-spots. To appreciate these, we must have recourse to the exquisite drawings of Langley, the truthful quality of which is recognized by every astronomer who has observed sun-spots under favorable conditions. We shall see that the characteristic structure represented by these drawings is repeated, on a far greater scale, in the higher regions of the solar atmosphere disclosed on recent spectroheliograph plates.

Since the time of Sir John Herschel, many astronomers have proposed vortex theories of sun-spots. One of the first of these is the theory of Faye, who supposed the whirling motion to be the direct result of the peculiar law of the sun's rotation. This law was discovered by Carrington, who found from observations of spots near the equator that the sun completes a rotation in about twenty-five days, while the motion of spots at a latitude of 40 deg. indicated the time of rotation to be nearly two days longer. Thus, as the rotation period increases toward the poles, the photosphere at the northern and southern boundaries of a sun-spot must move at different velocities (assuming the law of the sun's rotation to be the same as that of the spots). This difference in velocity would tend to set up whirling motions, clockwise in the southern hemisphere and counter-clockwise in the northern hemisphere. Sun-spots, in Faye's opinion, are the visible evidences of such whirls.

This theory has had many supporters, but it is now generally agreed that the difference in the rotational velocity of adjoining regions of the photosphere is not nearly sufficient to account for the observed phenomena. Secchi, one of the most assiduous observers of solar phenomena, was strongly opposed to Faye's theory. He pointed out that about 6 per cent of the spots he observed gave some evidence of cyclonic action, but in the vast majority of cases such forms as Faye's theory seemed to demand were lack-

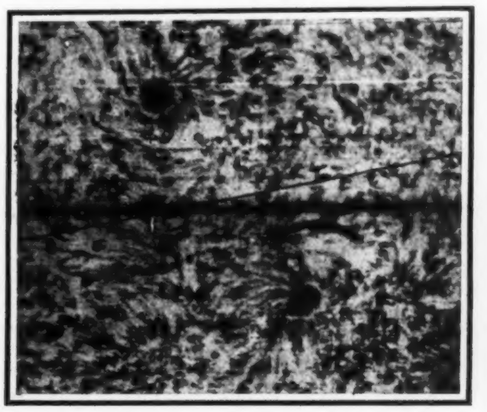


FIG. 4.—SUN-SPOTS AND HYDROGEN FLOCCULI, SHOWING RIGHT- AND LEFT-HAND VORTICES.

1908, October 7, 7h 02m A. M. Pacific standard time.

ing. We nevertheless owe to Secchi a most striking drawing of a sun-spot vortex.

When the spectroheliograph was first systematically applied to solar research in 1892, many rival theories of sun-spots occupied the field. Since the function of this instrument is to photograph the phenomena of the invisible solar atmosphere, it might be hoped that

* Discourse delivered at the Royal Institution.

* William Herschel "On the Nature and Construction of the Sun and Fixed Stars," p. 20.

the results would throw much light on the nature of sun-spots. For many years, however, this hope was not realized. The first monochromatic images of the sun were made with the K line of calcium. If we compare such an image with a direct photograph of the sun, made in the ordinary way, we see that the sun-spots are surrounded and frequently covered by vast clouds of luminous calcium vapor. These attain elevations of several thousand miles above the sun's surface, but they must not be confused with the prominences, which ascend to much higher elevations. When observed at the sun's limb, the bright calcium flocculi, as these luminous clouds are called, are so low, in comparison with the prominences, that they can hardly be detected as elevations. Thus our knowledge of the calcium flocculi must be derived mainly from the study of spectroheliograph plates, which show them in projection on the disk. I must not omit to mention, however, that the calcium vapor rises to the highest parts of the prominences, and that this higher and cooler vapor frequently indicates its presence on spectroheliograph plates in the phenomena of dark flocculi. These are relatively inconspicuous, however, and need not be discussed here.*

It soon appeared that the average photograph of bright calcium flocculi could not be counted upon to indicate the existence of definite streams or currents in the solar atmosphere. In 1903 the hydrogen flocculi were photographed for the first time. By comparing these flocculi with the corresponding calcium flocculi we see that, in general, dark regions on the hydrogen image agree approximately in form with bright regions on the calcium image. This might appear to indicate that hydrogen is absent in the regions where calcium is most abundant. An investigation of the question, however, does not lead to this conclusion. Dark hydrogen flocculi seem to mark those regions on the sun's disk where hydrogen is present as an absorbing medium, which reduces the intensity of the light coming through it from below. In certain areas, where the temperature is higher or the condition of radiation otherwise different, the hydrogen flocculi are bright. In many cases eruptions are in progress at these points, but in others the difference in brightness is apparently not the direct result of eruptive action.

The hydrogen flocculi, thus photographed with the lines $H\beta$, $H\gamma$, or $H\delta$, differ in many respects from the calcium flocculi. Not only do they usually appear dark, where the calcium flocculi are bright: their forms exhibit striking peculiarities, which are absent or much less conspicuous in the case of calcium. The appearance of the calcium flocculi resembles that of floating cumulus clouds in our own atmosphere; their capricious changes in form reveal the operation of no simple law. But the hydrogen flocculi, on the contrary, exhibit a definiteness of structure in striking contrast to this appearance. Some of the photographs strongly remind us of the distribution of iron filings in a magnetic field, and suggest that some unknown force is in operation.

Such was the condition of the subject when the red $H\alpha$ line of hydrogen was first applied to the photography of the flocculi, on Mount Wilson, in March, 1908. The calcium and hydrogen flocculi had been studied for several years, and much had been learned as to their nature and their motions. It had been found, for example, that the calcium flocculi observe the same law of rotation that governs the motions of sun-spots, while the hydrogen flocculi apparently follow a different law, in which the decrease in the angular rotational velocity from the equator toward the poles is much less marked. The latter result is in harmony with the investigations of Adams, whose accurate measures of the approach and recession of the hydrogen at the eastern and western limbs of the sun offer but little evidence of equatorial acceleration on the part of this gas. For this and other reasons it had been concluded that the hydrogen shown in such photographs reaches a higher level than the vapors of the bright (H_2) calcium flocculi. The region of the atmosphere previously explored with the spectroheliograph was nevertheless confined (except in the case of eruptions and dark calcium flocculi) to a comparatively low level, lying within a few thousand miles of the photosphere. What might be expected if a still higher region could be satisfactorily photographed in projection on the disk?

The red line of hydrogen offered the means of disclosing the phenomena of this higher atmosphere. As it may not immediately appear why different lines, caused by the radiation of the same gas, should not give precisely similar photographs, a brief reference to the aspect of a prominence in the red and blue hydrogen lines may be advantageous. Here are two photographs of the same prominence, seen in elevation at the sun's limb, one made with $H\alpha$, the other with $H\delta$. As the red line is very bright, even in the highest regions, the photograph taken with its aid shows the entire prominence. $H\delta$, on the other hand, is relatively weak at the higher levels, and consequently only the lower and brighter parts of the prominence are well recorded when this line is used. If, now, we suppose ourselves immediately above such a prominence, at a point where we observe it in projection against the disk, it is evident that the character of the hydrogen lines must depend upon their brightness at different levels. As we know that, speaking generally, absorption is proportional to radiation, the amount of light absorbed in the upper part of the prominence will be much greater for $H\alpha$ than for $H\delta$. Hence the average level represented by the absorption of $H\alpha$ will be higher than the average level represented by $H\delta$, since the higher gases play a more important part in the production of the former line. We may therefore expect that photographs of the sun's disk, taken with the light of $H\alpha$, will show the dark areas corresponding to absorption in the prominences much more clearly than photographs taken with $H\delta$. Moreover, since $H\alpha$ is stronger than $H\delta$ in the upper chromosphere, in regions where no prominences are present, the average level represented by this line will, in general, be higher than that represented by $H\delta$. A comparison of two photographs of the sun's disk, made with the lines in question, will suffice to make this clear. This enormous group of prominences, stretching for several hundred thousand miles across the sun, is much more clearly indicated by $H\alpha$ than by $H\delta$. In general, the hydrogen flocculi are stronger and more distinct when photographed with $H\alpha$, and there are some regions which appear bright with $H\alpha$ and dark with $H\delta$. This latter peculiarity probably has an important bearing upon the similar behavior of hydrogen in certain stars and nebulæ, but a discussion of this question cannot be undertaken here.

The first of the $H\alpha$ photographs gave strong hopes of a substantial advance in our knowledge of the solar atmosphere. The sharpness and comparatively strong contrast of these flocculi, and the evidences of definite structure and clearly defined stream lines which they revealed, were highly encouraging. The work was begun during the disturbed weather of the rainy season, when the definition of the solar image is never of the best. On April 30th, 1908, the first photographs were secured under the fine atmospheric conditions which prevail in the dry season. A direct photograph (Fig. 1) shows a small insignificant group of sun-spots, which would not seem, without other indications, to merit special attention. The next photograph (Fig. 2) shows that an enormous calcium flocculus occupied this region of the sun, but its form was in no wise remarkable, and afforded no evidence of the phenomena brought to light by the $H\alpha$ photograph. The structure recorded with the aid of the latter line (Fig. 3) recalls Langley's sun-spot drawings, and suggests the operation of some great force related to the sun-spot group. The same cyclonic structure had been less satisfactorily recorded on the previous day, but a comparison of the two photographs failed to indicate such changes as motion along the apparent stream lines might be supposed to produce.

The close of the rainy season now permitted an active study of the $H\alpha$ flocculi to be undertaken. Many photographs were made daily, and the almost constant association of apparent cyclonic storms or vortices with sun-spots became evident. During several months of the year in California an unbroken succession of clear days can be counted upon, so that the changes of a given vortex can be followed without interruption. The cyclonic storms were found to be of two principal types, the first associated with groups of spots and represented in such photographs as those of April 30th and September 2d, the second associated with single spots, and resembling a simple vortex, as illustrated in the photographs of September 9th and October 7th, 1908 (Fig. 4). The appearance of these simple vortices is such as to indicate rotation in a clockwise direction in the southern hemisphere, and in a counter-clockwise direction in the northern hemisphere (assuming the direction of motion to be inward toward the spot). However, this cannot be taken as a general law, corresponding to the law of terrestrial cyclones. Indeed, many instances have been found of closely adjoining spots, in the same hemisphere and frequently in the same spot-group, having magnetic fields of opposite polarity, produced by vortices rotating in opposite directions.

In some cases, at least, these vortices seem to exercise a powerful attraction on the surrounding gases, as a series of photographs taken on June 3rd, 1908, illustrates. A long dark hydrogen prominence, first photographed in elevation at the sun's limb on May 28th, had advanced half-way across the solar disk. It lay at the outer boundary of a well-defined vortex, centered on a sun-spot. This spot had been gradually separating into two parts, and on June 3rd the separation was complete. The first photograph of a series of nine was made on this day at 4h. 58m. Several successive photographs indicated no appreciable change, but one taken at 5h. 07m. showed that the prominence was developing an extension toward the spot. At 5h. 14m, this had assumed the appearance illustrated in the next photograph, and eight minutes later, when the last photograph of this series was taken, the extension had almost reached the spot. It will be seen that it divided into two parts, which indicates that each umbra was a center of attraction. The average velocity of the motion toward the spot was more than 100 km. per second. Later photographs, made on the following days, show a ring of bright hydrogen surrounding the spots, suggesting that the comparatively cool hydrogen carried down into the spots was re-heated and returned to the surface, after escaping from the lower end of the vortex. We thus seem to be observing some of the phenomena of an actual vortex in the sun; but it must not be supposed that cases of this kind are common. In many instances the hydrogen flocculi do not appear to move rapidly toward or away from spots, but undergo changes of intensity, as though the physical condition of the gas were constantly changing; but before proceeding further with a discussion of these sun-spot vortices, let us turn to another phase of the subject, which will afford much new information indispensable for this purpose.

We are all familiar with the effect produced by passing an electric current through a wire helix. The lines of force of the resulting magnetic field are parallel to the axis of the helix, and its intensity is determined by the diameter of the helix, the number of turns of wire, and the strength of the current. We also know, from Rowland's experiment, that the rapid revolution of an electrically charged body will produce a magnetic field. Thus, if a sufficient number of electrically charged particles were set into rapid revolution by the solar vortices, a magnetic field should result. What warrant have we for assuming the existence of charged particles in the sun, and how could such a field be detected?

Let me pass rapidly in review a series of phenomena with which you are all familiar. Sir William Crookes showed in this lecture-room, so long ago as 1879, that the negative pole of a vacuum tube sends out a stream of particles, capable of setting a light windmill in rotation, and deviated from their straight path when under the influence of a magnetic field. He has kindly consented to show the same tube again to-night; you now see the effect upon the screen. The recent work of Sir Joseph Thomson and others has proved that these are negatively charged particles, called "corpuscles" or "electrons," and that their mass is about 1/1700 of the mass of an atom of hydrogen. Moreover, Thomson has shown that at low pressures these corpuscles are given off from a hot wire or from the carbon filament of an incandescent lamp. He has also demonstrated that this property of emitting corpuscles at high temperature is common to carbon and to metals, whether in the solid or in the vaporous condition. Thus we have warrant for the belief that the sun, composed of just such elements as constitute the earth, must emit great numbers of these corpuscles. As Thomson has estimated that the rate of emission of a carbon filament at its highest point of incandescence may amount to a current equal to several amperes per square centimeter of surface, we can hardly be mistaken in assuming the existence of still more powerful currents in the sun. The emission of negatively charged particles implies the emission of positively charged particles, but in laboratory experiments, because of unequal rates of diffusion or other causes, charges of one sign are always found to be in excess. We thus have reason to believe that powerful magnetic fields may result from the revolution of these particles in the solar vortices.

In seeking a means of detecting such fields, let us first recall Faraday's discovery of the effect of magnetism on light, made at the Royal Institution in 1846. This discovery relates to the rotation of the plane of polarization of light when passed through a plate of dense glass in a strong magnetic field. Although Faraday, in what was said to be his last experiment, endeavored to detect the effect of magnetism on the lines of the spectrum, he failed because the apparatus then available was not sufficiently powerful. In 1896 Prof. Zeeman examined with a large spectroscope the two yellow lines emitted by sodium vapor in a flame between the poles of a powerful magnet. Observing in the direction of the lines of force, he saw that the sodium lines widened when the magnet was excited. Subsequently, with more powerful apparatus, he found that a single line, when observed under the above conditions, is split into two components by a magnetic field. The distance between the two components is a measure of the strength of the field; but the most characteristic quality of these double lines, which distinguishes them from double lines produced by any other known means, is the fact that the light of the two components is circularly polarized in opposite directions. If, then, we encounter a double line in the spectrum of any substance, and suspect it to be due to a magnetic field, we must apply the test for circular polarization.

The simplest means of testing for circularly polarized light is to transform it into plane polarized light

* Eruptive prominences are also recorded on the disk as bright flocculi.

by passing it through a quarter-wave plate or a Fresnel rhomb. In the case of a Zeeman doublet, we would then have issuing from the rhomb the light of the two components polarized in planes at right angles to one another. A Nicol prism, standing at a certain angle, will transmit one of these plane polarized beams and cut off the other. Turning the Nicol through 90 deg. will cause the component previously cut off to be transmitted, and the other to be stopped.

Consider a sun-spot at the center of the solar disk, and suppose it to be produced by a vortex, the axis of which lies on the line passing from the eye of the observer through the spot to the center of the sun. In these circumstances, if a strong magnetic field is produced by the vortex, the spectral lines due to vapors lying within this field should be widened or transformed into doublets. Moreover, the light of the components of these doublets should be circularly polarized in opposite directions. This would be true if the spot vapors were emitting bright lines, identical in character with those emitted by a radiating vapor between the poles of a magnet. The experiments of Zeeman, Cotton, König, and others, show, however, that dark lines, produced by the absorption of the spot vapors, should behave precisely in the same way as bright lines.

The spectrum of a sun-spot was observed for the first time by Lockyer in 1866. He found that many of the lines of the solar spectrum were widened where they crossed the spot, and the observation of these widened lines has been carried on systematically by many observers ever since. Conspicuous among these observers was Young, whose last observations were made with a powerful grating spectroscope attached to the 23-inch Princeton refractor. This instrument showed that some of the spot lines are close doublets. Dr. Walter M. Mitchell, who at first worked in conjunction with Prof. Young, and later by himself, gave special attention to these double lines, which he found to be particularly numerous at the red end of the spectrum. He called them "reversals," and the existing evidence favored the view that they were produced by the radiation of a hotter layer of vapors overlying the spot, which would give rise to a narrow bright line at the center of the widened dark line.

True reversals of this kind actually seem to occur in the case of *H* and *K* and other lines in the spot spectrum, and it was therefore natural that Mitchell should attribute the similar phenomena of the spot doublets to a similar cause. It was generally supposed that the widening of the dark lines was due to the increased density of the spot vapors. The diverse character of the lines in the sun-spot spectrum is well illustrated by this drawing, which is due to Mitchell. In addition to the ordinary widened and "reversed" lines we find cases where a dark central line is accompanied by wings, others in which lines are thinned or completely obliterated, etc.

(To be continued.)

SCIENTIFIC GUESSWORK.

THE VALUE OF LOOSE METHODS.

In scientific research, as in less ambitious affairs, the possession of a good imagination is often of the utmost value. We may go even further than this, and say that on occasion it is the only means available for advancing our knowledge into unexplored regions. For in all pioneer work there comes a time when the strictly logical reasoning of the textbooks fails to carry the investigator forward, and he must turn to his imagination to supply some missing step or to suggest a fresh pathway for exploration. The tentative nature characteristic of scientific deductive reasoning in so many branches of inquiry is alone sufficient testimony to the extent which the imaginative powers play in building up our knowledge. The practical application of this mental quality is seen at almost every turning among the steps that lead to all great discoveries and inventions. There are few instances in which a closely reasoned sequence of ideas has been employed in the evolution of outstanding achievements; there are equally few in which the authors of these advances have not owed a large measure of their success to the happy inspiration of their imaginations. But there is a limit to such methods as to every other, and care must be taken to guard against an overindulgence in them. Work, otherwise excellent, can be easily spoiled by a too ready departure from the regions of orthodox procedure.

The tediously gathered results of investigation have before now been rendered barren of any real good to the scientific world by the looseness of deduction employed afterward. However much trouble we may take in order to secure accuracy in our observations, they are useless until they have been carefully correlated among themselves and with the results obtained by others. Yet it is not uncommon to find some of our best and most careful workers making a loose and even careless analysis of the results of their experiments. Thus assumptions are made which, their nature being afterward forgotten, are taken as truths; arguments are drawn from slender hypotheses and theories built all too readily on purely fanciful grounds. It is imagination run rioting and degenerated into mere guesswork. By such means science can never advance, and may in time incur ridicule rather than respect by becoming the playground of fancy. No more pointed illustration of this tendency could be obtained than is afforded occasionally by the graphical results of experimental work. Relying, as it would seem, mainly on intuition, a curve will be drawn through the plotted points, and it will then be asserted that the result represents the law sought. Inspection all too frequently shows that not only is the "law" thus indicated honored in the breach, but is greatly dishonored in the observance. What sort of mental jugglery the authors indulge in so that they may become convinced of the correctness of their views, or how they can impose them on others, it is difficult to guess. For in the cases referred to many curves besides that given might be drawn and with equal justification. The curve shown has frequently no more claim to be considered as that which would be obtained if experimental errors were absent, than a host of others. Yet two curves possessing the same degree of probability may have vastly different shapes and a consequently great influence on any theory or practical work founded on them. As an example of this rash jumping to a conclusion, we would instance certain experiments carried out a short time ago on wind pressures. Here the plotted points were joined up with a smooth curve which was thought to represent best the true state of affairs. Near the origin the divergence of some of the points from the curve thus drawn was seen to be excessive, but it was concluded that this was due to unusually great experimental errors. Some time later further work on the same lines yielded results which showed that at this region the curve had points of inflection and should dip down so as to include the discredited points of the former experiments. Thus the entire theory built on the first results had to be modified, and a new phenomenon was discovered, which, although in evidence in the earlier work, had been passed over because of the wrong assumptions made. At other times we have seen experimenters boldly drawing in curves when the number of points on the diagram was wholly insufficient to warrant any curve whatever being given. A striking example of this was published recently. Several points were clustered in two opposite corners of the figure, a group lay in the middle, and a curve approaching a paraboia in shape—but it might just as readily have been a quadratrix or a caustic—was drawn through the points, virtually only three in number. A superfluity of points may likewise lead to guesswork of this nature. We need only refer to experiments carried out on the strength of columns to show a typical example of this class. In certain charts giving the experimental results of an investigation on this subject, the curve threads its path through a "milky way" of points. Some are far above it, some are far below it, and those which fall on it are insignificant in number compared with those that do not. It cannot be pretended, then, that the curve represents the results of practical research, for the diagram itself shows that the column is much more likely to fail when loaded to some other stress than that indicated by the curve.

It is difficult to suggest any general means whereby this tendency to guesswork may be eradicated or even modified. A certain amount of it is, and always will be necessary in all scientific research work. The great difficulty is to place a limit to its indulgence, to determine at what point it ceases to be justifiable and becomes a hindrance in the pathway of advancing knowledge. Each particular example, it is plain, should be judged on its own faults. Yet we may state as a general proposition, covering the most flagrant examples, that excessive departure from strict reasoning is frequently due to the influence which the worker's former beliefs exert on his mind when he is analyzing the results of his research. Many experimental investigations would, we believe, be greatly benefited by an honest attempt on the part of their authors to get rid of preconceived ideas. For it is seldom that an experimenter commences to work with a perfectly open mind as to how the results should turn out. When these are found, as they frequently are, to be somewhat different from the results forecast, there are few workers who are sufficiently strong minded to believe firmly in their own figures. After the experiments are over the results will be modified and "corrected" so as to bring them more into line with what the investigator thinks they should be. By tampering in this fashion with his figures he may readily obscure certain features of the subject investigated which are really inherent in it and not, as he thinks, accidentally present. In the same way it is easy for him to read into his results features that have no practical existence and thus found a totally erroneous theory. Where the conditions of the experiment are thoroughly understood we are, no doubt, justified in altering our figures to a limited extent in a direction coincident with that in which our reason tells us truth must lie. This is the true use of imagination. But where these conditions are imperfectly known, where there are no previous experimental facts to guide our reason, we must, to avoid mere guessing, abide closely by our own results until such time as further work will enable us to view them in a fuller light.—The Engineer.

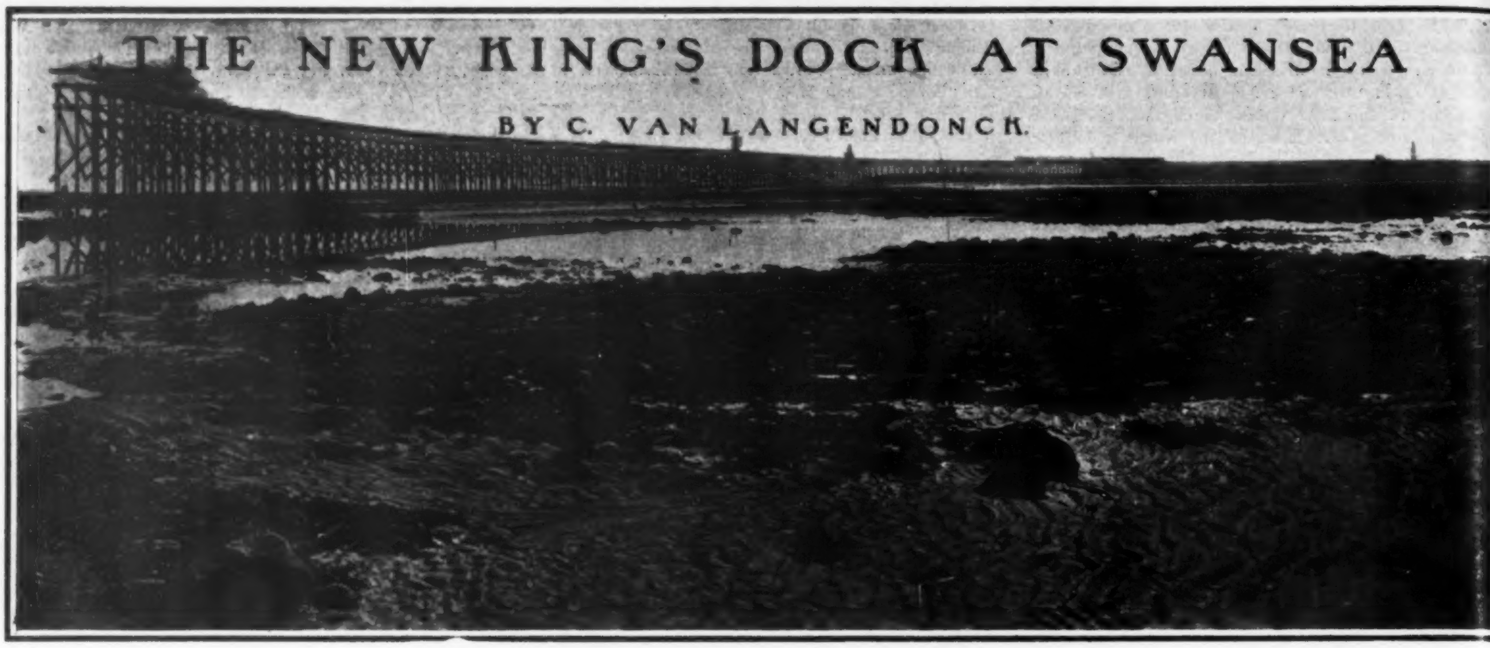
EXOTIC DAINTIES.

An astonishing variety of opinions, in regard to what is fit to eat and what is especially delicious, is found among the different races of mankind. Articles which appear strange and repugnant to Europeans appear as dainties at Chinese and Japanese banquets, and some African tribesmen greedily devour grasshoppers, and even termites and scorpions. The writer recently held the empty but still fragrant tin of his last German "hand cheese" under the nose of a Samoan, who was astounded by this evidence of the white man's fondness for foul-smelling articles of food. Yet many strange and horrible things are eaten in Samoa. If a fisherman feels hungry he pulls the fine-spines from a freshly caught fish and devours all the rest. The flesh of the holothuria, or sea cucumber, is eaten, although it is gritty with carbonate of lime, but the tough leathery skin and the contents of the almost hollow body are rejected. The forests are infested by the larvæ of a gigantic stag beetle, caterpillars four inches in length and as big as a man's thumb, which feed on fallen and decaying trees. The natives dig out the caterpillars with knives and hatchets and eat them alive, with the exception of the head. A favorite and particularly repulsive dish, the eating of which is now prohibited by the authorities, is rotten breadfruit. The crop of breadfruit is so great that it cannot be consumed in the fresh state, so the breadfruit is burned in pits lined with banana leaves until it has become quite putrid and is then eaten. It must be admitted, however, that the Samoan bill of fare contains many things which are keenly relished by European palates.

A new type of diving dress, especially adapted to deep-water operations such as cylinder sinking and submarine salvage, has been devised by an English engineer, Mr. Henry Parker, of Tring Hill, Hertfordshire. This engineer has been identified with many difficult cylinder sinking undertakings in connection with bridges, notably the River Murray bridge in Australia, where cylinders were bedded in granite 96 feet below water, and the Forth bridge. The diving plant which he has now evolved is based upon the experience gained in the foregoing tasks. The diving dress is almost as flexible as the ordinary attire, but enables the diver to carry out his work in 200 feet of water with greater ease than at present attends his movements at a depth of 80 feet. Moreover, it is doubly safe in moving among rocks or the interiors of sunken ships, whether in deep or shallow water. The diver can come up from a depth of 200 feet to the surface, and lie on the water for a time as the pressure is reduced as desired, without incurring any injury and with less risk than is at present the case. The cost of the dress does not exceed that of the ordinary outfit by more than 25 per cent. The usual pump and all existing lighting and speaking connections can be attached to the helmet, as in the present installation.

THE NEW KING'S DOCK AT SWANSEA

BY C. VAN LANGENDONCK.



THE TIMBER GANTRY FOR MAKING SEA EMBANKMENT.

Although the port of Swansea is placed on the banks of the river Tawe, it may for all practical purposes be regarded entirely as an artificial port, the discharge of the river being trifling as compared with the volume of tidal water ebbing and flowing between the pierheads. The history of the Swansea port works may be said to have commenced about 1792, when the construction of a number of piers was begun. At the beginning of the present century the total docks and basin work carried out since the earlier date covered an area of about 5 acres, and included five docks and three basins.

The staple trade of the port of Swansea is coal, and the continued growth of the tonnage entering the port during the latest years rendered it imperative that some considerable addition to the existing dock accommodation should be provided, and after having obtained the necessary parliamentary powers, the Swansea Harbor Trustees decided to construct a new dock, which should be sufficiently large to meet all the requirements of the port for many years to come, and should have the necessary accommodation for receiving the largest vessels that are likely to be built.

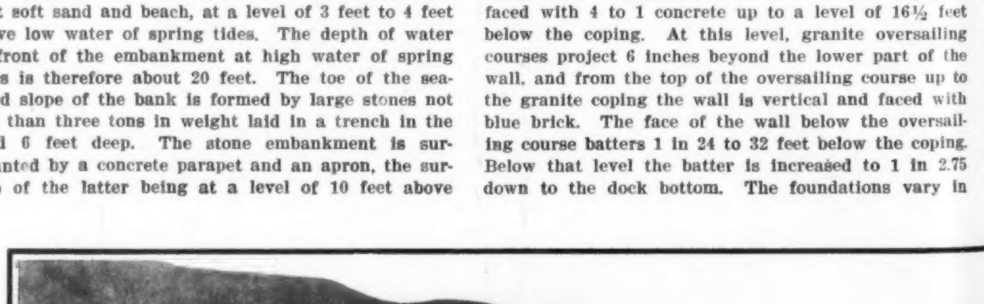
The principal trade of the new dock, which was commenced in July, 1904, will be in coal, and the arrangements of the berths, sidings, and approaches to the quays have been specially designed with this class of traffic in view. The total cost of the gigantic work, including coal-handling apparatus, warehouses, and machinery, amounts to about $10,000,000.

The new dock, now called the King's Dock, possesses a water area of 8 acres, in addition to the area of the deep-water extension, which when it is completed will add 40 acres to the total. The dock is irregular in shape, and the entrance lock is placed so as to facilitate swinging. The largest vessel the lock can take will be able to swing in the dock near the lock without interfering with any ship berthed alongside the quays. The width of the dock at this point is 1,240 feet. The extreme length is 4,600 feet, the width in the upper portion or coaling arm, which is 4,000 feet long, averaging 560 feet. The length of the quays reserved for general trade is 10,350 feet, and for coaling purposes 2,800 feet, making a total of 13,150 feet. In addition, there are pitched slopes forming the dock sides for a length of about 400 feet, which can be converted into wharves. The level of the impounded water is 35 feet above the dock bottom and 6 feet below coping level. The level of the water will be maintained by means of pumping when the increase in the number of lockings between spring tides has the effect of materially lowering the water level in the dock. Provision will be made for increasing the depth of water in the dock by 2 feet.

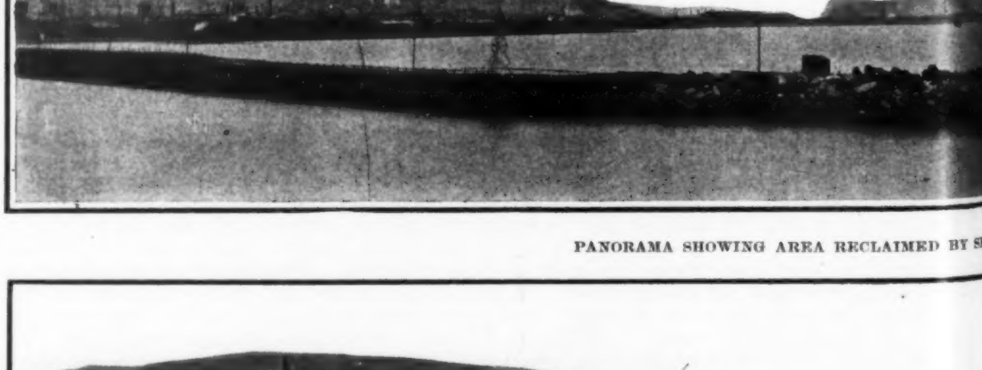
The most striking feature of the planning of the dock is the exceptionally large proportion of quay frontage to water area. This has been obtained without unduly restricting the water area required to enable vessels to maneuver or to lie at moorings on the dock awaiting a berth. The peculiar arrangement of projecting jetties in the coaling arm is designed with the view to economy of berthing space. The bays between the jetties, which vary in length from 280 feet to 493 feet on the south side, will be occupied by vessels of moderate length. These will be loaded by means of the movable coal hoists, which will traverse as required to take up positions opposite the ship's hatchways. Other vessels, the length of which may exceed the limits permissible in the bay berths, will lie alongside the jetties overlapping the ships moored in the bays alongside the wharves. The ships at the jetties will be loaded by means of the fixed hoists, and warped in or out as necessary to bring the hatchways under the tips. This device practically doubles the wharf accommodation, and is, we believe, a novel arrangement in dock construction. The obvious criticism is the possibility of difficulties in mooring a large vessel broadside on to the end of a narrow jetty when a strong gale is blowing. The engineers have, however, provided heavy moorings at intervals along the middle of the dock. The mooring anchors consist of large concrete blocks sunk below the surface of the dock bottom. The mooring chains are shackled to mooring bars imbedded in the concrete.

For constructing the dock, the first work undertaken was the reclamation of 400 acres of foreshore by means of a sea embankment. Within the area so inclosed the dock itself was constructed later. The sea embankment is nearly two miles in length, and consists of a stone rubble bank protected on the sea face by large stone pitching, no stone in the pitched face being less than two tons in weight, the general average being about four tons. The stones were carefully set by two 8-ton capacity titan cranes. The foreshore at the site of the embankment is for the most part soft sand and beach, at a level of 3 feet to 4 feet above low water of spring tides. The depth of water in front of the embankment at high water of spring tides is therefore about 20 feet. The toe of the seaward slope of the bank is formed by large stones not less than three tons in weight laid in a trench in the sand 6 feet deep. The stone embankment is surmounted by a concrete parapet and an apron, the surface of the latter being at a level of 10 feet above high water of spring tide. The parapet wall is 7 feet high, and shelters the reclaimed area from sea wash and spray. The method of construction of the sea embankment was as follows: A timber gantry, which averaged over 30 feet in height from the foreshore to the rail level, was constructed on the foreshore along the center line of the embankment. Traveling pile-driving stages, each carrying three piling engines, were started from two points and worked in both directions. Erecting gangs kept the construction of the gantries well in advance of the rubble mound. Train loads of stone rubble were brought on to the gantries and tipped until the latter structures became buried in the stone *débris*. The piles and other timber, with the exception of the upper portions of the gantry, were left buried in the rubble mound as the work proceeded. A little portion of the embankment was constructed without a gantry, the materials being tipped end on as the bank advanced. A total of about 528,000 cubic yards of stone has been used in the construction of the embankment, in addition to 1,213,000 cubic yards of sand from the dunes.

The dock walls, as well as those of the lock, are constructed throughout of 6 to 1 cement concrete, faced with 4 to 1 concrete up to a level of 16½ feet below the coping. At this level, granite oversailing courses project 6 inches beyond the lower part of the wall, and from the top of the oversailing course up to the granite coping the wall is vertical and faced with blue brick. The face of the wall below the oversailing course batters 1 in 24 to 32 feet below the coping. Below that level the batter is increased to 1 in 2.75 down to the dock bottom. The foundations vary in



PANORAMA SHOWING AREA RECLAIMED BY SE... ATER INC...



PANO... DOCK.

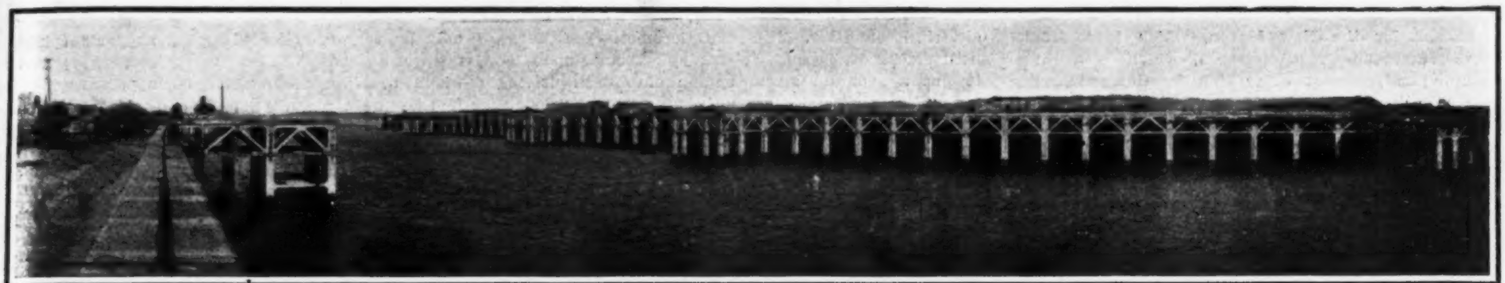
THE NEW KIN... AT

depth; in places they were carried as low as 28½ feet below dock bottom, making the maximum height of the wall 69½ feet. The base width of the wall is 22 feet, which is reduced by steps to 7 feet under the coping. For the purpose of draining the ground at the back of the walls during construction, two 4-inch cast-iron pipes are built into the wall at every 75 feet.

These drain pipes were sealed on the completion of the work. Vertical dry rubble-stone drains are laid behind the walls at the back of the drain pipes, and a continuous dry drain is placed at the level of the lowest cast-iron drain pipe. In those parts of the dock where no quays have been provided, the sides have been sloped to 1½ to 1 and stone pitched. The stone-pitched slope is capped by a granite coping set on a concrete foundation. The material to be excavated from the dock consisted principally of sand and ballast, but in places considerable deposits of clay and numerous large boulders were encountered. Nearly the whole of the excavation was done by steam excavators, several types of these machines being used. The material excavated amounted to 4,500,000 cubic yards.

On the north side of the coaling arm the quay for a length of 750 feet is built of ferro-concrete on the Hennebique system. The same construction has been adopted for the coaling wharves on the south side of the arm, 1,138 feet in length, together with five jetties, which increase the total frontage of the ferro-concrete work on that side to 1,413 feet. This method of construction was adopted to obtain a greater width of firm foundation, required for both the movable and fixed coal hoists, than is possible with solid concrete walls. The ferro-concrete wharves are more costly than the solid wall when compared by the cost per foot of frontage, but there is a saving by the adoption of the former method when the cost of the heavy foundations for the hoists and crane roads at the back of the wall is added to the cost of the latter.

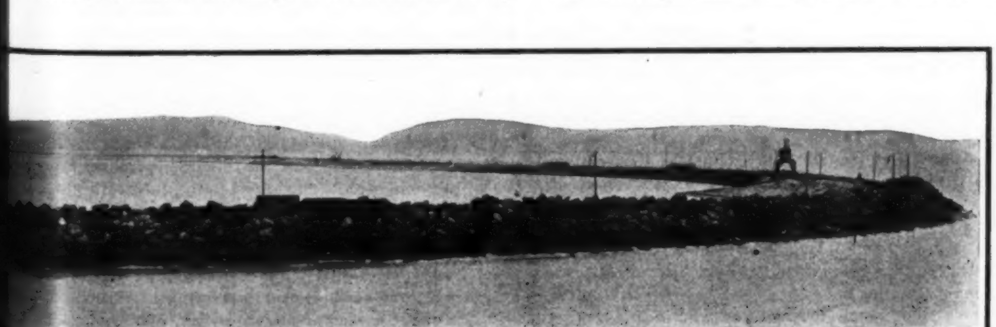
As said, the jetties fronting the south wharf are five in number, and all are 55 feet wide. The projection of the jetties into the dock varies, the middle one, which may be taken as typical, having a projection of 50 feet on the west face, while on the east face the projection is 115 feet, the difference being due to the *echelon* arrangement of the wharf faces and jetties. The construction of the ferro-concrete wharf on the north side follows the design of the wharf on the opposite side of the dock. The superload on the wharves is calculated at 2 hundredweights per square foot of deck in the way of the movable hoists, in addition to the actual load imposed by the hoists themselves, which is 304 tons for each hoist. The front beam of the wharf, which is 2 feet 4 inches by 1 foot 2 inches in section, carries a moving load of 200 tons on four wheels, the total wheel base being 36 feet. The remainder of the moving load of the hoist, viz., 104 tons, is carried by the back beam, measuring 3 feet 2 inches by 1 foot 8 inches. The deck elsewhere than between the movable hoist tracks is calculated for a super load of 3 hundredweights per square foot. The jetties which carry the fixed hoists and the viaducts leading to them are calculated on the same basis as the wharf work, the total load of each hoist being taken as 340 tons. The columns of the hoists are carried on 3½ feet square ferro-concrete pillars.



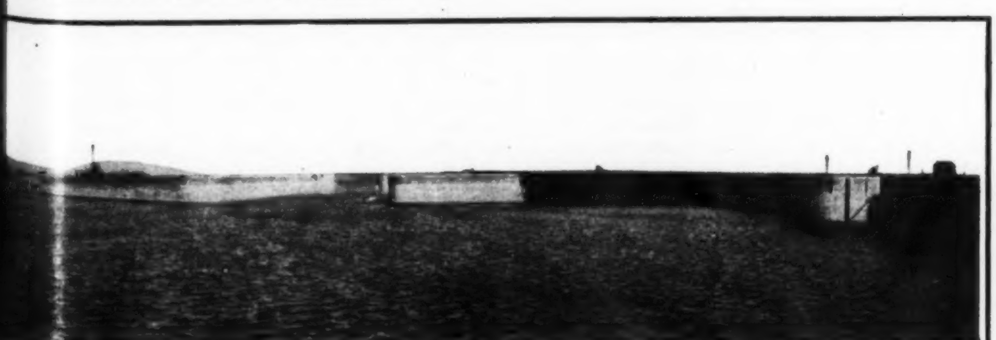
FERRO-CONCRETE COALING WHARF. VIEW SHOWING FERRO-CONCRETE COALING QUAYS.

As already stated, a large portion of the eastern part of the King's Dock is to be devoted to coal traffic, and sections of the coaling wharves have been leased to railway companies by whom they will be worked. When the coaling scheme is complete, there will be a total of seventeen fixed and movable hoists provided in this part of the dock.

The entrance lock is placed at the southwestern corner of the King's Dock, and is 876 feet long between the pointing sills of the inner and outer gates, and 90 feet wide at coping level. The dock is subdivided by intermediate gates into two compartments, respectively 500 and 375 feet long. The main sluices, which are placed in shafts over the main filling and emptying culverts, 6 by 10 feet, are raised and lowered by direct-acting hydraulic presses. In addition to the six main sluices, the lock is provided with four floor sluices, 4 by 6 feet, for use in clearing the gate chamber floors of any accumulation of mud. The lock is furnished with eight hydraulic capstans, four of which are capable of exerting a pull of 7 tons, and the other four of 5 tons direct from the barrel. The three pairs of gates are generally of steel and wrought iron, of the water-borne type, and are alike in design, each gate being divided by means of two watertight decks and a central vertical watertight bulkhead into five compartments, the lowest of which extends the full length of the gate, and is in free communication with the outer water through openings in the bottom deck. The two compartments above this form the air chamber, and the remaining two are water chambers free to the outer water through open scupper pipes at the top deck of the air chamber, the position of this deck being arranged so as to insure a proper excess of weight over buoyancy at all states of tide.



ED BY SE[A W]ATER INCLOSED, BEFORE BEING SLUICED OUT.



PANO[RAMA OF] DOCK.

[COAL]ING AT SWANSEA.

THE PARIS FLOODS AND THEIR PREVENTION.

ALTHOUGH Parisians have breathed more freely since the subsidence of the floods, they have, nevertheless, not been without considerable anxiety. During the month the Seine has frequently risen to a level that in ordinary times would be looked upon as dangerous, and protective works have been hastily carried out in the shape of concrete walls to prevent the water from spreading in the event of its overflowing the banks. The presence of the danger has necessarily given special prominence to the measures which it is proposed to carry out for preventing the recurrence of any such disasters in the future. The problem is so complex that the commission which has been appointed by government to go thoroughly into the question includes engineers, architects, scientists, members of the various departments of public works, deputies and senators, and, in fact, everyone possessing special experience enabling him to throw some light on the matter. If anything, the commission appears a little unwieldy for dealing with a problem that requires urgent solution. Under the presidency of M. Alfred Picard, it has drawn up a programme which covers no fewer than nineteen subjects, which are each to be reported upon by a specialist. Among the subjects are the following: Methods of predicting floods by improving the service of observation in the Basin of the Seine; embankments and parapets in Paris; influence of the Paris sewers on the floods; the effect of the bridges on the inundations; measures to be taken for the protection of the railways, the Metropolitan and the tramways, and for preventing any interruption to the telegraph and telephone service, as well as to the distribution of electricity, gas and compressed air; the relation between floods and the destruction of forests, and the carrying out of works for preventing any abnormal rise of the Seine level, especially so far as concerns Paris and the suburbs.

While, as will be seen from this outline, the programme of the Commission on the Seine Floods deals mainly with protective works, it is obvious that the most interesting feature of the problem lies mainly in the measures of prevention. If it is possible to prevent the floods, even at considerable expense, it may prove more economical to do so than to erect protective works. It is difficult, for instance, to see what can be done to prevent the sewers from playing such a disastrous part in the floods, unless they are entirely reconstructed. Wherever they burst they tore up the roads, and even when the floods had subsided the falling in of a road was, for the time being, an almost daily occurrence. Besides, protective measures would apply to Paris alone, while anything which can moderate or prevent the overflowing of the Seine would be of incalculable benefits to a vast population in its valley, and might eventually be the means of putting an end to the perennial and destructive floods of the Rhone and the Loire. So far as Paris is concerned, a proposal has been made to widen and deepen the moat of the abandoned fortifications on the north side of the city, so as to constitute a canal which will relieve the Seine on its passage through Paris, where, in times of floods, it is partially dammed by the bridges and embankments. A similar project, it appears, was put forward fifty years ago, and was again suggested in 1872 by Belgrand, who pointed out that floods were bound to take place periodically. Another scheme aims at preventing floods altogether by sinking a number of "absorption wells" at suitable places in the basin of the Seine. These wells would be sunk in porous strata which would absorb all the water flowing into them, and thus, as soon as the tributaries began to overflow their banks, the surplus water would sink in the subsoil. As this system is successfully employed for reclaiming marsh land, it is one which will probably meet with due consideration from the commission. In view, however, of the tremendous volume of water which has been passing through Paris it would appear as if this system of

regulating the level would necessitate a very extensive and costly system of wells. Much has also been said concerning the destruction of forests, and doubtless a good deal is to be attributed to this factor, but there is a prevailing idea that while the reconstruction of forests is eminently desirable, it will not alone solve the problem of regulating the water level. There is a considerable difference of opinion as to the value of the various schemes put forward, but absolute unanimity as to the necessity of doing something without delay to save Paris from another disaster.—The Engineer.

HEAVY ELECTRIC LOCOMOTIVES.*

SOME MODERN DEVELOPMENTS

BY C. L. DE MURALT

Except for the New Haven, which is a single-phase, and the Great Northern, which is a three-phase pinat, almost everything in the trunk line electrification in this country has been of the continuous, or direct-current, type. In Europe it is nearly otherwise, almost everything except one or two small installations having been of the three-phase, alternating-current type. The difference is mainly a commercial one. The American engineers have shown just as much ingenuity and resourcefulness as European engineers; but the American manufacturer is given to exploiting one particular standard piece of apparatus just as soon as that piece of apparatus can be consistently sold, and can be made to do its work. That means that American engineers in general are particularly encouraged in applying standard apparatus to the conditions which they meet, rather than in designing a new piece of apparatus for each particular case. In Europe, standardization was, until a very few years ago, practically unknown. Even small motors were hardly what we call standardized, and each year every factory would turn out a new line of motors, and not only a new line, but they might change the 5 horse-power and the 25 horse-power motor, and perhaps afterward the 15 and 20 horse-power. Each installation was treated as a new problem, and for that new problem a new piece or pieces of apparatus were designed. This explains why, in Europe, progress in electrification matters was much quicker than in America. Each installation, each road, was a distinct improvement on the one before, and there are no two exactly alike, while in America, on the contrary, we find one or two new ones, and then ten or twelve or more modeled after those in the standard fashion.

The continuous-current equipment has long been with us here, as well as in Europe, in street-railway practice, and therefore a natural thing was to use continuous-current apparatus for the first trunk-line electrifications. When it came to long-distance work, everybody recalls the limitations of low pressures which are an essential feature of continuous current equipment. Therefore two methods were developed for overcoming these limitations. One was to continue using continuous-current apparatus in the motor cars and locomotives, but to transmit the energy at high pressure alternating current, and then to transform in the sub-stations into continuous current. The other method was to use alternating currents directly in the motors, the transmission being essentially the same.

American engineers prefer the first method; European engineers choose the latter. The reason for these two different choices is, again, a commercial one. The American engineer had before him a well-developed series of standard continuous-current motors, and he was loth to give that up for something which to him appeared as something entirely new. He had plenty to do to develop the sub-station part of the alternating-current and continuous-current equipment, and therefore he chose the alternative. The European did not have a standard line of continuous-current apparatus, and therefore, as he had to design a new equipment anyway, it was just as easy and just as logical for him to pick out the alternating current, which offered several advantages, the principal one at the time appearing to be the elimination of the rotary converter sub-station, which we will all admit is not a particularly agreeable adjunct to a trunk-line system. This explains why, in Europe, all trunk-line electrifications to date are of the three-phase alternating-current kind, while in America, all but a very few are of the continuous-current kind.

At this date, however, both countries are getting to imitate each other. The Americans are adopting the good points of European practice, and the Europeans have already adopted a good many points of American practice. I should like to point out one or two special points which I call modern developments in trunk-line electrification and heavy electric locomotives from the American standpoint. In Europe they have both been used for some time.

The first one is the coming use of the side rods in electric locomotives. The steam locomotive, of course, used side rods, because it was just about impossible to put a separate steam engine onto each driving axle. When electric locomotives were first brought out, it seemed a very natural thing to do to get away from the side rods, because the electric motor lent itself so admirably to subdivision, and it was the easiest thing in the world to put an electric motor on each driven axle. Everybody was very happy to do away with the side rods, because they were considered part of the reciprocating equipment of the steam locomotive which causes pounding and other disagreeable features, deteriorating the track, and causing a certain swaying of the locomotive. By and by, it was found, however, that the side rods were not so bad as would appear at the first glance. It is necessary to distinguish between different portions of the driving gear as used in the steam locomotive. You have certain parts which are plainly reciprocating, like the piston, the piston rod, the crosshead, and a few others. They cannot be counter-balanced. They will have a direct motion to and fro, and to eliminate those parts in the electric locomotive was a great step forward. The side rod pure and simple, however, which connects the first driver with the second and third, etc., is not, strictly speaking, a reciprocating part. If you follow the motion of the side rod in one revolution, you see that it has rotary motion, and if you put a counterweight onto the other end of your crank-shaft, and thus compensate for the weight of the side rod, you will find that the side rod will not have any hammer-blow effect; in other words, it can be accurately counterbalanced. Therefore, if the side rod has any advantages, it is not necessary to eliminate that; and the side rod has very valuable features. The first one is that it allows the raising of the center of gravity of the locomotive. You may think that that is not an advantage, because when electric locomotives were first brought out, it was claimed for them as a particular advantage that their center of gravity was lower than the center of gravity of the steam engine, but, as is so often the case, this was found to be an error of judgment, and a very low center of gravity is not an advantage, but a disadvantage. If you go to the other extreme, and raise the center of gravity very high, as is the case in the most modern steam locomotives, you hurt the design, and the locomotive gets to be top-heavy. But between that and the very low center of gravity of the earlier electric locomotives, there is a vast difference. A very low center of gravity was so hard on the track that it was really a mistake to place the center of gravity quite as low as it used to be placed in the beginning. Therefore, as soon as this fact was realized—and, unfortunately, it was realized only after two or three accidents caused by the heavy effect of the low center of gravity on the rails—the designers immediately started to find means for raising the center of gravity. So long as the motors were each one directly connected with its axle, it was difficult to do much in this line. You will have seen pictures or drawings of some locomotives where the motors are raised and are put in a slanting position, not only for raising the center of gravity, but, incidentally, for bringing the center of gravity higher than it would be if they were placed horizontally. But there were very small limits to this way of attacking the problem. Practically the only, and by far the best method is to place the motor directly on the frame of the locomotive. It might be still connected by gears as the early motors were, but that would be rather a difficult proposition. It is very easy, however, to connect the motor by means of side rods, to place the motor on the locomotive frame, and then connect it by means of side rods with the driving axle; and this is one advantage which the side rods present, the possibility of placing the motor at whatever point in height is considered best.

The second advantage, and probably just as valuable, is the fact that by means of side rods the entire weight of the motor can be spring-supported. Before side rods were used, the motors were connected by means of gears, or directly to the axle. If they were directly connected to the axle, it was only by very complicated spring supports (placed in the wheels, like the New Haven locomotive) that the weight could anywhere nearly be supported by springs. If they were gear-connected, one-half the weight of the motor could easily be spring-supported. The other half, however, bore down directly onto the axle. Whatever portion of the weight was not spring-supported had, of course, also a very hard effect on the track. Therefore, the use of side rods, and with it the raising of the motor on top of the locomotive frame, where it could be entirely spring supported, together with the frame, was a step in the right direction.

A second modern development is the abolition of the commutator. You all know that a continuous-current motor must have a commutator. A commutator is not a serious proposition so long as the motor is used in laboratory, or, for that matter, in shop work, where it receives careful attention and periodic inspection, and where it is not subjected to overloads of any serious nature. The railway motor, however, works under entirely different conditions. It runs through all kinds of weather. It gets wet, and hot, and dusty. It is not inspected any too often, even on the most careful roads. It is frequently quite habitually subjected to overloads, and therefore it is really handicapped from the start, and its commutator is the point which shows the bad effect first. The windings of the field and of the armature can be protected against all these bad treatments, but the commutator is an exposed point. Commutators will spark at best, but if they are poorly treated, they will spark badly. That means that the brushes will be eaten up, and then the commutator itself will be eaten up, and this takes place to such an extent that almost any railroad, be it street railway or trunk line, which uses continuous-current motors has a great amount of its reserve for motor repairs expended on the commutator. The percentage varies. I do not believe it is ever less than 40 per cent.; it is mostly above 50 per cent. and it goes as high as 80 per cent. of the total repairs for repairs on the commutators alone. Therefore to get away from the commutator is, indeed, interesting. You cannot abolish the commutator unless you use an alternating-current motor. You cannot do it even with a single-phase alternating-current motor because the single-phase induction motor is not, in its present stage of development, suitable for railway work. It will not present the proper starting characteristics. The single-phase series-motor has, of course, a commutator, because it is practically only an improved continuous-current motor. The only motor which can do without the commutator is the three-phase alternating-current motor; and therefore the use of the three-phase alternating current, in addition to all the other advantages, presents the one great and financially interesting advantage of eliminating the commutator.

* A lecture given before the Worcester Polytechnic Institute Branch of the American Institute of Electrical Engineers, Worcester, Mass.

In a paper on "The Mechanics of Dust," read before the Society of Engineers by Mr. C. H. W. Biggs, the author says one of the most interesting investigations, and one open to every student, is that of the effect of minute air currents upon dust particles. Imagine for a moment dust particles having a very large surface compared with their mass. Fortunately, the student can on occasion easily see such particles, and watch their evolutions, in a room where otherwise it is almost, if not quite, impossible to detect air currents by other means. Smoke consists of small particles of matter, and is in daily use to detect minute currents of air both in drains and in rooms. There is an excellent way to investigate and to follow the motion even of individual particles. Dr. Travis, in his experimental work on seeing colloidal matter, makes great use of the ultra-microscope. You can get something of this effect in rooms where a window faces a bright sun. Darken the room, leaving only a hole—preferably circular—through which to get a beam of bright sunshine. Thus we get a cone of brilliant light through the room. In this cone you can see dust particles playing all manner of antics, some falling, some rising, some going in one direction, some in another, the direction of the small masses being the direction of the resultant of the forces acting.

PARADOXES OF ACCELERATION.

A SUGGESTION FOR THE TEACHER OF MECHANICS.

BY C. E. GUILLAUME.

Among the very subtle notions which often embarrass the teacher of mechanics, none more frequently leads to erroneous interpretations than the two aspects in which acceleration may be considered, but there is also none which, when once mastered, is more fertile of results in thorough instruction. The nature of the question will appear clearly on consideration of the following problem: If a railway tank car is stopped on an incline, the surface of the liquid will evidently be horizontal; but if the brake is released and the car allowed to move freely, the friction being supposed negligible, what form will the surface of the liquid then assume? If this question should be asked of a number of persons expert in mechanical problems, few would be found able to answer without prolonged consideration. Hence this problem, simple though it appears, is a difficult one. We shall associate with it a number of similar problems, which will enable us to understand it better.

It is not possible for everyone to experiment with the tank car, but every reader can walk through an ordinary railway car at the moment of starting or stopping. He will then observe a singular fact. At the moment of starting the floor of the car will seem to be raised in front, and at stopping it will appear to be raised behind. In the Paris subway, where the accelerations are considerable, this phenomenon is very striking. A passenger walking forward at the moment of starting experiences the sensation of ascending a steep grade. This illusion, the explanation of which we shall see later, foreshadows the solution of our problem, but we must make still another digression in order to attack the problem without difficulty.

Every body which is free to move is caused to fall vertically by the attraction of the earth. If the body is prevented from falling, it exerts upon its support a pressure which we call its weight. Now what is the weight of a body which is falling freely? This is a difficult question, to which an erroneous reply is often given. A very simple process of reasoning will show us the truth. Let us suppose that two coins are laid, one upon the other, on a board, and that the board is suddenly snatched away. As long as the lower coin was supported by the board, the upper coin pressed with all its weight upon it. Does the same condition prevail during the fall of the coins? Certainly not. In fact, each coin falls, with an acceleration equal to g, under the action of its own weight. If we suppose that in these conditions the upper coin continues to press upon the lower one, it is evident that the latter will be subjected to a total force which is greater than its own weight, while the upper coin will experience a downward force less than its weight. Hence the acceleration of the lower coin will be greater, and that of the upper coin will be less than g, and, consequently, the coins will immediately separate. As this reasoning remains true, no matter how small the mutual pressure of the coins is supposed to be, we see that while falling freely, the coins cannot exert any such mutual pressure.

These facts are evident enough, yet heresies abound in this field, and some very celebrated ones have been accepted almost without comment. The novelist Jules Verne, in order to remove his travelers to the moon from the action of gravity, carries them to that point in space in which the attractions due to the earth and the moon counterbalance each other. This expedient was unnecessary. The projectile in which the travelers were inclosed, would have fallen freely toward the earth as soon as it was free to move. Although its speed of ascent was very great, its downward acceleration would have preserved its normal value. As the passengers fall with the vehicle there would be no mutual attraction between them and it, and they would be able to move with a freedom unhampered by gravity, without having to wait until they reached a certain point of space.

The complete verification of this statement is unfortunately impossible, but partial proofs can be found without difficulty. The commencement of the descent in a mine elevator is accompanied by a marked feeling of discomfort, which is due to the fact that the acceleration, although very much less than that of gravity, is large enough to destroy a considerable proportion of the pressures which the various organs of the body normally exert upon each other. The stomach, suspended in midair as it were, unmistakably indicates that it does not relish these conditions of existence; and if the action continues, or is repeated frequently, as it is on a rough sea, this organ makes an energetic protest.

Now let us see what will happen to a pendulum if its support yields to the attraction of gravitation. A process of reasoning analogous to the above shows that, if the descending movement begins at the moment when the pendulum has attained its greatest elongation, it will remain in this inclined position as long as it continues to fall freely. If the fall begins during the oscillation of the pendulum, the latter will rotate in a vertical circle with a constant angular velocity. These results might be expressed by saying that as the weight of the pendulum is employed in causing it to fall, it cannot also cause an oscillation; this will be asking too much of it. It will be more exact to say, however, that, as the pendulum experiences no reaction from its support, it is not subjected to any mechanical couple, and consequently has no tendency to modify its rotation.

This reasoning explains why clocks are little affected by the attraction of the sun and moon. If the moon revolved about a motionless earth, clocks would run slow when the moon is in the zenith, and would run fast when it is in the nadir. But in fact

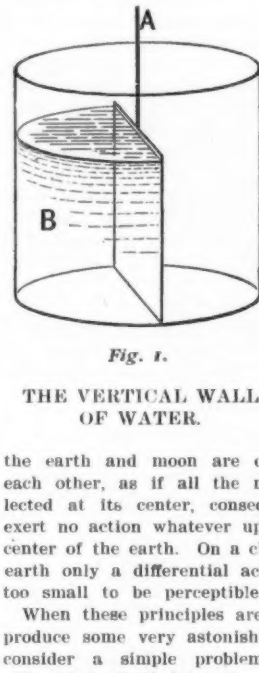


Fig. 1.

THE VERTICAL WALL OF WATER.

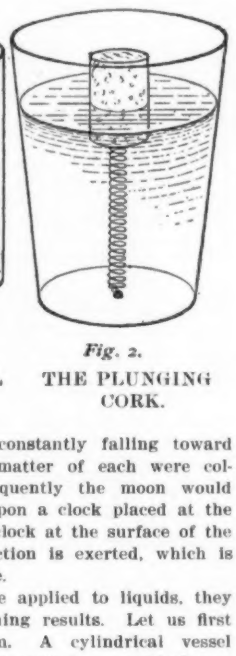


Fig. 2.

THE PLUNGING CORK.

the earth and moon are constantly falling toward each other, as if all the matter of each were collected at its center, consequently the moon would exert no action whatever upon a clock placed at the center of the earth. On a clock at the surface of the earth only a differential action is exerted, which is too small to be perceptible.

When these principles are applied to liquids, they produce some very astonishing results. Let us first consider a simple problem. A cylindrical vessel (Fig. 1) is divided into two parts by a vertical partition passing through its axis. The part on one side of the partition is filled with water, while the other part is empty. The vessel is allowed to fall and at the same instant the partition is removed, leaving to the water absolute freedom of movement. What form will the water assume? The answer is simple. The water will maintain its form unchanged, i. e., it will continue to fill one-half of the vessel, preserving a vertical face, as if the partition had not been removed. The reason is that the water and the vessel are both falling freely, and are thus removed from the action of gravitation.

A still more surprising result can easily be obtained by an experiment devised by Prof. G. de Metz, who repeats it annually in his course of lectures at the University of Kieff, and who recently communicated it to me. In a glass of water (Fig. 2) floats a cork, attached to the bottom of the glass by a spiral spring, the tension of which is only sufficient to increase slightly the immersion of the cork. If the glass is dropped, what will the cork do? The answer is very surprising. The cork will plunge deeply into the water. The truth of this assertion is apparent upon a little reflection. We learn from the preceding experiment that water exerts no pressure upon the vessel which contains it, when both are falling freely. For the same reasons, the various strata of the liquid exert no mutual pressure, or in other words, there is no pressure in the interior of the liquid. Now as the rise of the cork to the surface is due to this internal pressure, in the absence of the latter the cork will necessarily plunge downward under the impulsion of the spring, which is the only force acting upon it. At the moment when the glass touches the floor, gravitation reasserts itself. A considerable pressure is developed in the interior of the mass of water, and the cork emerges suddenly.

For the reasons which have been indicated above, the bubbles which rise in sparkling wine cease to rise if the glass is dropped to the floor, and rise more rapidly if it is knocked against the table.

Now let us return to the problem of the railway car. Experimenting upon one's self is not always a rapid process of discovery. Let us rather drop a small coin from the ceiling of the car at the exact moment of starting. It is certain that the coin will actually fall in a vertical line, but, as the car is in motion, the coin will appear to descend obliquely. Hence the horizontal plane which is perpendicular to this line of fall will appear to descend toward the front of the car, or in other words, the floor of the car will appear to be raised in front. But the experiment with our own body teaches us a little more, namely, that the real acceleration of the train is combined with the virtual acceleration of gravity (the acceleration which gravity would communicate to us were it not for the reaction of the floor of the car) to form an oblique resultant, which gives us an erroneous idea of the position of the vertical line. In free fall, as we have seen above, the idea of verticality ceases to exist. In Jules Verne's projectile, it would have been quite impossible to decide, by the muscular sense alone, the direction in which the earth was situated. The preceding problems are connected together by this modification of the idea of up and down.

The problem of the tank car remains to be solved. In this case there is free acceleration in the direction of motion and, in this direction, the notion of force ceases to exist. The only component which can make itself felt by a mutual action between the liquid and the car, is that component of which the real acceleration is annuled by the reaction of the rails. Hence the surface of the liquid will place itself at right angles to this direction, i. e., parallel to the rails, just as if the car were at rest on a level track.

An application of this principle immediately suggests itself. Attempts have been made to construct grade indicators for automobiles and aeroplanes, but the results hitherto obtained with such devices have been very unsatisfactory. This is because the virtual acceleration of gravity, which acts along the true vertical, is combined with the real acceleration in the direction of motion and, as the latter often predominates, it makes the indications of the instrument absolutely nugatory. If the instrument, furthermore, contains parts which are free to move in the direction of the motion of the car, all rotating parts are affected by centrifugal acceleration, as well as by the variations in the slope of the path.

This is also the reason why altitudes measured with a sextant, held in the hand on the deck of a vessel, require to be reduced, either to the true horizon or to an artificial horizon, such as is given by the Fleurlais top. The level of the sextant furnishes an instantaneous horizon, i. e., the plane perpendicular to the resultant of the real and virtual accelerations.

Hence we see that the same principle reappears in many fields, and, when it is not perfectly understood, apparently simple problems produce great perplexity. Several of these problems are of great practical importance and deserve serious study.—La Nature.

Indelible Pictures (France Pat.).—One or both sides of the picture (on paper, linen, silk, wood, etc.) should be covered with a thin, translucent sheet of pyralin, celluloid, or other composite substance, having pyroxyline for its chief constituent. The picture can be covered only on one side, the front, or on both sides with a sheet of pyralin. In the latter case, as only the pyralin sheet on the front side needs be transparent, the back sheet can be of any desired color. The sheet or sheets of pyralin are first prepared in the familiar manner and attached to the picture by moistening its surface with alcohol, or some other solvent of pyroxyline, and then applying warmth and pressure. We can also use a transparent cement or mucilage to fasten the leaves, or no solvent at all, employing only heat and pressure. A back of card or wood may be applied to the picture, in which case the pyralin sheet is applied only to the front of the picture. After the picture has been firmly fastened between the pyralin sheets they can be polished in the ordinary manner, i. e., by pressing them against a highly polished metal plate, or they can be polished first and then fastened to the picture.

A NEW METHOD OF REVERSING TURBINES.

FOETTINGER'S TURBINE PROPULSION SYSTEM WITH TURBO-TRANSFORMERS.

The application of turbines to ship propulsion commends itself by advantages such as uniform torque and strains, avoidance of vibrations, simplicity of operation, saving in lubricants, comparatively small wear at bearings, and the possibility of "forcing" the engine, that is, driving it at abnormally high speeds. Yet the turbine has not been able to supplant the reciprocating steam engine in ship propulsion. There are two reasons for this fact:

First, no practical reversible turbine has been de-

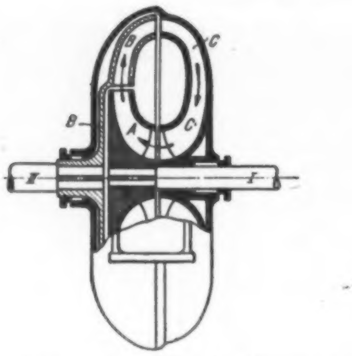


Fig. 1.—TYPE OF TURBO-TRANSFORMER.

signed so far, and since it is necessary that a ship should be capable of backing astern, separate (normally idle) turbines had to be added for this purpose; as generally constructed, these reversing turbines have a maximum power amounting to from 30 to 45 per cent of that available for forward propulsion, whereas reciprocating engines allow from 70 to 90 per cent of the power available for forward propulsion to be utilized for backing, at all speeds. A ship fitted with reciprocating engines can therefore be stopped and reversed more quickly than a turbine ship of the same power.

Second, screw propellers are most efficient at a rate of speed considerably lower than the rate of speed which is best for efficiency and economy of turbine operation (from five to fifteen times lower).

In most constructions devised hitherto, a compromise has been adopted, that is to say, the propeller and the turbine shaft have been given the same speed, intermediate between those at which each of these elements would yield the best results. As a consequence, the efficiency of the propeller has been reduced, while the weight of the turbine has been increased.

If it be desired to operate propeller and turbine at their proper economical speeds, the interposition of speed-reducing mechanism between them becomes imperative. Toothed gearing as proposed hitherto is asserted to be unsuitable for high speeds or for transmitting high powers; moreover, it does not reverse. Electrical gearing would be too heavy and too bulky for practical purposes. Hydraulic transmission of a crude form has been proposed, the turbine driving a centrifugal pump which propels a body of liquid against the buckets of a wheel (say a Pelton wheel) whose shaft carries the propeller. This transmission has a low efficiency, chiefly on account of losses in the propulsion of the circulating liquid.

To overcome these drawbacks, Dr. H. Föttinger of Stettin, Germany, assisted by the Vulcan Works, has invented and developed a system of propulsion for which he claims not only a high degree of efficiency, but reversible action without mechanical complications. The feature of this invention is a very compact and relatively light and simple hydraulic transmission device, the so-called turbo-transformer.

The accompanying diagrams illustrate two types of the new turbo-transformer. In each view, *I* indicates the drive shaft, that is the turbine shaft, to which is secured rigidly the rotary pump *A*. Independently rotatable is the driven shaft or propeller shaft *II*, carrying rigidly the water wheel *B*. The parts shown in solid black are stationary and contain guide channels *C* which together with the buckets of the pump *A* and of the wheel *B* form a closed circuit for the water or other liquid employed as the transmitting medium. In the first diagram, the pump throws the liquid direct into the water wheel, from which the liquid returns to the pump through the guides *C*; in the second diagram, the liquid passes from the pump first through the stationary guide channels and then to the water wheel, from which it has a direct return to the pump.

The energy imparted to the liquid by the pump *A* is absorbed largely by the water wheel *B*, and inasmuch as the liquid returns to the pump immediately, there is very little loss of energy; the energy remaining in the returned liquid reduces the energy required to operate the pump, or in other words, the load on the turbine shaft. By properly selecting the dimensions of the parts, the desired ratio of speed is obtained between drive shaft and driven shaft.

Dr. Föttinger has devised buckets and blades specially adapted for the new use. In practice, the types shown in the diagrams have been modified and combined in various ways; more particularly an intermediate wheel, rotating independently, has been interposed between the pump and the driven wheel carried by the propeller shaft. Turbo-transformers with one or more intermediate stages are advisable when a considerable reduction of speed is desired.

Dr. Föttinger has noted the possibility of using his invention to increase speed instead of reducing it.

This new system of propulsion offers very marked advantages. First of all, the apparatus is very compact, which involves a saving in space, weight, and cost, and in view of the fact that the power-transmitting liquid circulates in a short path, losses due to friction and whirling are reduced to a minimum. The inventor further avoids the considerable losses which in the crude form of hydraulic transmission above referred to are due to the double conversion of energy, first from velocity to pressure, and then back from pressure to velocity. Again, the velocity which the

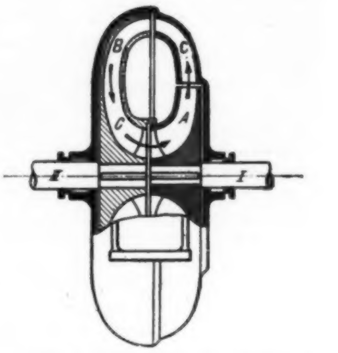


Fig. 2.—TYPE OF TURBO-TRANSFORMER.

water still has when it leaves the driven wheel of the turbo-transformer is not wasted, but assists in propelling the liquid through the pump. The production of a vacuum and corrosion due to it are excluded by the continuous motion of the liquid in a direct path; moreover, if desired, a standpipe, tank, pump or other means may be connected with the circulating space to produce any desired pressure therein.

A point of chief interest, of course, is the efficiency of the new system. Calculations made by Dr. Föttinger before practical trials, led him to expect an efficiency of from 80 to 82 per cent in the case of large units and a speed reduction ratio of from 1/4 to 1/5. These calculations were based on well-known figures concerning the efficiency of rotary pumps and water wheels. The efficiency of standard rotary pumps runs from 80 to 84 per cent, and a low-pressure centrifugal pump devised by Dr. Föttinger has shown an efficiency up to 86 per cent. Water wheels of good construction have efficiencies of from 84 to 85 per cent, even with small units. These figures take into account the loss of energy at the outlet. In the turbo-transformer this loss is avoided, as the liquid re-enters the pump immediately; therefore, even if the loss at the outlet is assumed as low as 3 per cent, the pump of the turbo-transformer would

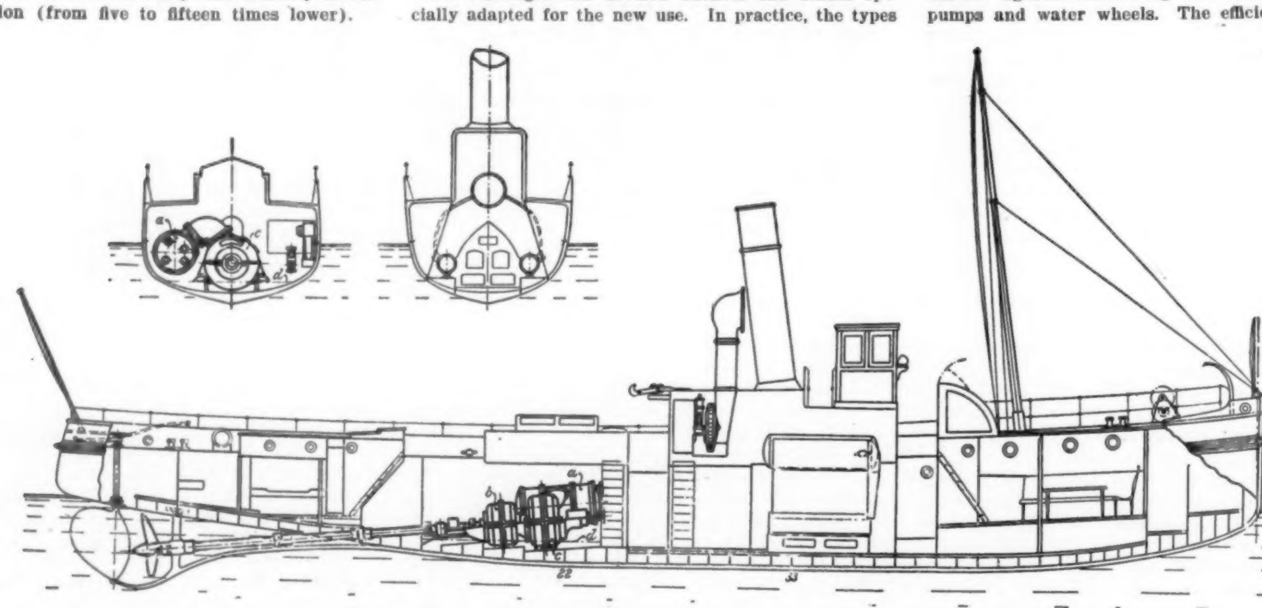


Fig. 3.—THE EXPERIMENTAL VESSEL.

have an efficiency of from 86 to 87 per cent, and the water wheel an efficiency of from 87 to 88 per cent. Since this pump consists only of a set of rotating buckets or blades, all the losses occurring in the other parts of an ordinary centrifugal pump are eliminated and the efficiency of the rotary pump member alone will be about from 93 to 94 per cent. When connected in series with the water wheel, this pump forms the turbo-transformer whose efficiency is equal to the product of the individual efficiencies, that is, from $0.93 \times 0.87 = 0.81$ to $0.94 \times 0.88 = 0.83$, or from 81 to 83 per cent. In practice, an even higher efficiency has been attained in some cases.

The efficiency depends on the reduction ratio. With a reduction ratio between 1/4 and 1/6, an efficiency of from 80 to 82 per cent is readily obtained, employing two reduction stages (that is, one intermediate stage). A speed reduction in the ratio of 1 to 8 is obtained with three stages and an efficiency of about 80 per cent. Calculations made by the Vulcan Works, in connection with the proposed installation of the turbo-transformer in rolling mills, indicate an efficiency of about 75 per cent with a speed reduction ratio of 1/11 or 1/12. Even higher reduction ratios may be employed in cases where absolutely uniform speed is not essential.

When a steam turbine is used to drive the turbo-transformer, the total efficiency of the plant can be increased further by recovering the waste heat. If the transformer has an efficiency of 80 per cent, this means that 20 per cent of the power applied to the pump is converted into heat, raising the temperature of the circulating liquid. With a direct drive, from the pump and a water wheel, is used for rotation in both directions, while the other part (in this case the guide channels) can be altered to reverse the rotation of the wheel. Some water wheels have buckets so formed that they can run in either direction. With such, reversing can be obtained by movable or adjustable guide channels, thrown to one position or the other by a hand lever or special mechanism. Or two connected sets of guide channels may be employed, one formed to produce forward rotation and the other to produce rearward rotation, and by shifting one set of guide channels or the other into operative position, rotation of the desired direction is obtained. In this second type, the circulating path remains full of water even upon reversal of the propeller.

Third: When very quick reversal is desired there may be used, in combination with an arrangement of the first or of the second type, a device for temporarily throwing one of the circulating paths out of action while leaving it filled with water.

Another important question is the adjustment required to change the number of revolutions of the propeller shaft, so as to alter the speed of the ship. With a constant speed reduction ratio of the transformer, the energy absorbed by the transformer varies as the cube of the number of revolutions. The same law (with close approximation) obtains as regards the screw propeller, and therefore when the turbo-transformer is applied to ship propulsion, the speed-reducing ratio of the transformer will remain constant without the provision of any governor. The number of revolutions of the propeller shaft will therefore be varied simply by adjusting the action of the steam uses such devices might be necessary or desirable, and designed different types, described in patents. These regulating devices may be of the same character as the reversing devices explained above, that is, adjustable blades or buckets, or guide channels arranged to be shifted into or out of operative position, or throttle valves, etc. Regulation may also be effected by varying the number of stages in operation. Thus by employing say only one stage for top speed, and say two stages for cruising speed, a higher speed reduction ratio will be obtained at cruising speed, while the steam turbine is operated at a practically constant speed. A transformer of this character is now building at the Vulcan Works.

Dr. Föttinger's turbo-transformer was first subjected to a thorough practical test in 1907. The driving engine was not a steam turbine, but an electric motor of 100 horse-power (capable of developing up to 182 horse-power), having 1,000 revolutions per minute. The driven shaft was intended to perform 225 revolutions per minute, that is, the speed reduction ratio was to be about 4.5 to 1. The turbo-transformer had two circulating paths, a two-stage path for forward rotation, and a one-stage path for reverse rotation. Leakage from one path to the other (about 1.5 per cent) was collected in a tank and returned by means of a replenishing pump. A piston valve controlled the circulation of the water through one path or the other.

The tests made with this experimental arrangement under varying conditions of motor power and load bore out Dr. Föttinger's calculations. The torque was found to rise considerably when the driven shaft was

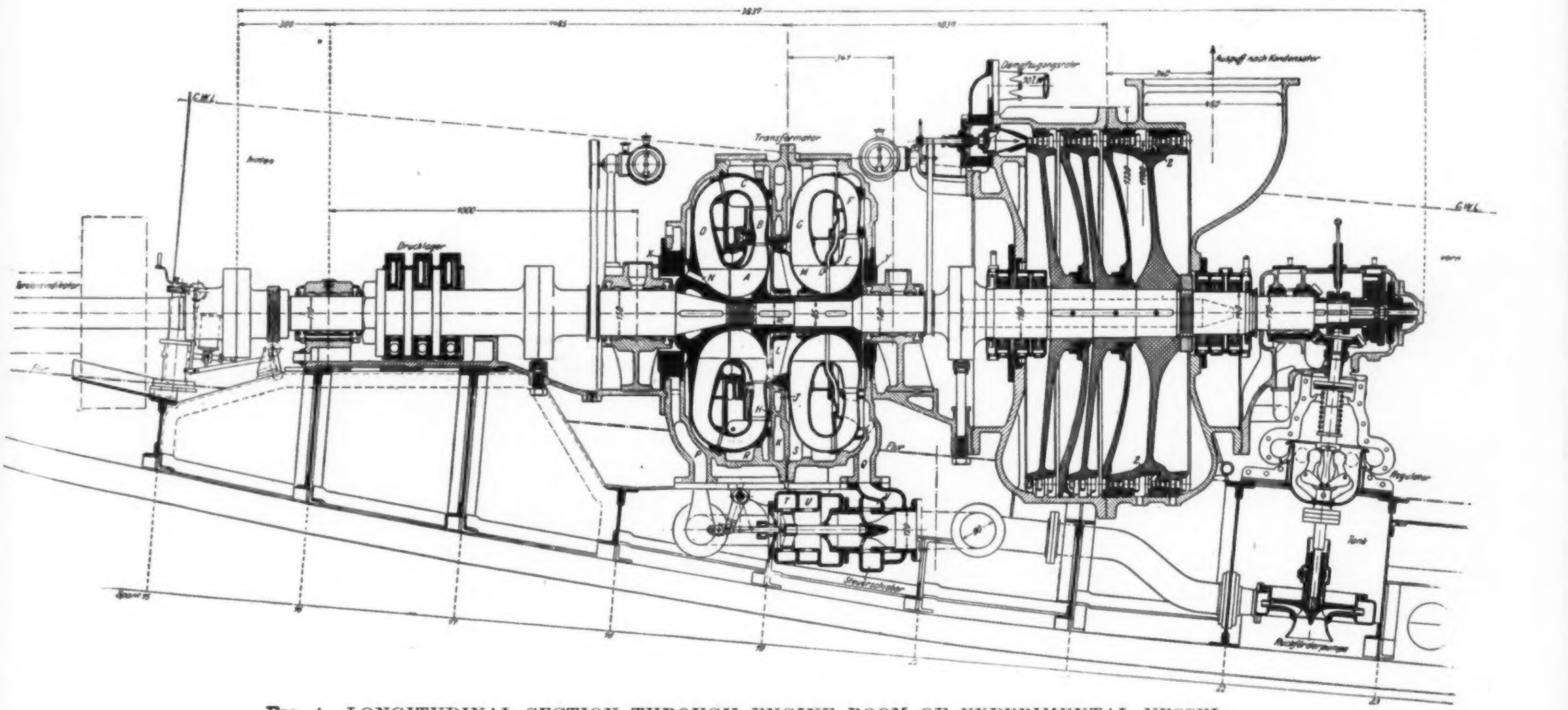


FIG. 4.—LONGITUDINAL SECTION THROUGH ENGINE ROOM OF EXPERIMENTAL VESSEL.

5 to 15 per cent of the engine power is wasted as heat absorbed by the sea; with the turbo-transformer, the heat developed in the transformer can be recovered almost totally and utilized, say for heating the feed water, temperature of which can thus be increased 20 or even 25 deg. C. without any consumption of coal for this purpose. The feed water may simply be made to circulate through the transformer, and will remain of a practically constant temperature notwithstanding variations of the load, since with a reduction of condensation water there is also a reduction of heat loss. Heating the feed water in this manner increases the steaming capacity of the boilers, and thus the power developed, to the extent of from 3 to 4 per cent, so that a transformer having an efficiency of 80 per cent may be made equivalent to one having an efficiency of 83 per cent.

A very important advantage of the turbo-transformer is that it can be readily made reversible, while the steam turbine always revolves in the same direction and is controlled by a governor. Three types of reversing arrangements are described by Dr. Föttinger:

First: The transformer has separate circulating paths for forward and rearward propulsion respectively. The temporarily idle path is emptied into a tank. The shifting of the circulating water from one path to the other can be accomplished with the aid of the powerful centrifugal pumps of the transformer, or a separate small auxiliary pump may be supplied to return water to the circulating paths and also to replace any leakage. A balanced slide-valve governs the admission to one path or the other. In the stopping position both paths are emptied.

Second: One part of the transformer, for instance turbine driving the pump. The efficiency of the transformer is also practically constant within a relatively wide range of speed.

It is well known that a steam turbine has its maximum efficiency at a determined number of revolutions. When the turbine is coupled direct with the propeller shaft, it is necessary, on account of weight considerations, to give the turbine (even at top speed) a number of revolutions which is from 10 to 30 per cent below the one of maximum efficiency. When operating at reduced speed (cruising speed), a considerable loss of efficiency occurs. With a turbo-transformer the steam turbine, at top speed, may be given a number of revolutions even exceeding that of maximum efficiency, so that a reduction of the ship's speed will actually for a time increase the efficiency of the steam turbine, and in any event, the reduction of turbine efficiency, from top speed to cruising speed, is much less with a turbo-transformer than with direct transmission. In a particular case investigated by Dr. Föttinger, the reduction of turbine efficiency from top speed to cruising speed was found to be 30 per cent for the direct drive and only about 16 per cent for the turbo-transformer system; thus, assuming both systems to be equally economical at top speed, the transformer system would show an economy of

$$\frac{(1 - 0.16) - (1 - 0.3)}{1 - 0.3} = 20$$ per cent over the direct drive at cruising speed.

While, as stated above, no device for regulating the speed of the transformer, and particularly for varying its speed reduction ratio, is required for the propulsion of ships, Dr. Föttinger realized that for other stationary, so that a quick starting of this shaft was obtained together with a rapid acceleration to normal speed. The maximum efficiency within wide limits of motor speed was secured with a speed reduction ratio of 4.25 to 1 (against a calculated ratio of 4.45 to 1). When keeping the motor speed constant (say at 1,100 revolutions per minute) and varying the rotary speed of the driven shaft, a maximum efficiency of about 83 per cent was found at 260 revolutions, corresponding to the speed reduction ratio just stated. When varying the power supplied by the motor (that is, its number of revolutions), the efficiency rose with an increase of power. With 5 horse-power the efficiency was more than 70 per cent; with 9½ horse-power, 75 per cent; with 40 horse-power, 80 per cent; with 100 horse-power, 82½ per cent; and with 182 horse-power, 83 per cent. When reversed, the turbo-transformer showed a maximum efficiency of 70 per cent, so that the power available for reverse rotation was about $70/83 = 85$ per cent of the power furnished for forward rotation. This proportion is from two to three times as large as that obtainable in practice with propellers driven directly by steam turbines. The proportion might be increased further by employing two or more stages in the circulating path employed for reversing.

In view of the satisfactory results obtained, the Vulcan Works proceeded to install the same turbo-transformer in a screw-propeller ship built specially for this purpose. This ship, of 76.7 tons, had a length of about 90 feet, a beam of about 14 feet, and a draft (including the keel of about 4 inches) of about 5 feet. The driving engine was a Curtis steam turbine with four wheels each having three rows of blades. Steam

was supplied by a water-tube boiler at a pressure of 17 atmospheres, the grate surface being 31 square feet and the heating surface 1,614 square feet. The shaft of the turbine carried directly the two centrifugal pumps of the turbo-transformer. Provision was made to balance the axial thrust. The replenishing pump required only about ¾ of 1 per cent of the turbine energy. The boiler and turbine were rated at 500 horse-power. The number of revolutions of the turbine was selected as 1,750; a speed reduction ratio of 4.5 to 1 would have required 390 revolutions per minute for the propeller shaft, but for special practical reasons the propeller was made larger than it would have been in most cases, and the number of revolutions decreased to about 318, which meant giving the speed reduction ratio the increased value of 5.6 to 1.

This ship developed a speed of from 12 to 13 knots and was used as a cargo boat, as an ice breaker, as a ferry, as a tug for towing a cruiser, etc. Its behavior was highly creditable, especially in heavy seas. The reversing of the propeller was particularly remarkable. With the ship going ahead at full speed, the reversing lever was thrown from "ahead" to "astern," the engine continuing to rotate in the same direction; the propeller shaft came to a standstill after four or five seconds, and within ten more seconds had acquired a reversed speed of from 200 to 250 revolutions. The stopping action was so powerful as to produce on the persons aboard a sensation similar to that experienced when the brakes are applied on a street car.

Dr. Föttinger has made elaborate calculations of the dimensions and efficiencies which would result from the adoption of his turbo-transformer in larger ships, up to 40,000 horse-power, and has also investigated the possibility of utilizing his invention for other purposes, as in rolling mills. He suggests that the turbo-transformer may find a profitable application in connection with gas turbines as well as steam turbines, or with internal combustion engines of the piston type, particularly in view of the fact that such engines are not reversible.

AEROPLANE STABILITY.*

SOME NEW THEORIES.

BY G. H. BRYAN.

In 1896 I had the pleasure of attending a lecture on naval architecture given before the British Association in Liverpool by the late Dr. Francis Elgar, F.R.S. I had learned the theory of the metacenter in my undergraduate days, but it came to me as a great surprise to learn that this theory had only been evolved after many ships had foundered, owing to want of theoretical knowledge of their conditions of stability.

I was interested in aerial navigation at the time, and although I had not got further than throwing gliders, it was evident from their behavior that a mathematical theory of stability must necessarily be of even greater importance in connection with aerial navigation than with naval architecture, and I wrote in Science Progress to the effect that if the future development of artificial flight were not to be a repetition of the chapter of accidents by which naval architects had gained their theoretical knowledge, there would be abundant work for mathematicians in reducing the conditions of stability to pure calculation.

About the year 1903 I noticed that if a glider or other body is moving in a resisting medium, such as air, in a vertical plane with respect to which it is symmetrical, the small oscillations about steady motion in that plane are determined by a biquadratic equation; and Prof. Love directed my attention to the condition of stability given by Routh. Mr. W. E. Williams was a post-graduate student in my department, and with his collaboration we published a paper on "The Longitudinal Stability of Aerial Gliders" (Proc. Royal Soc., lxxiii.), which was intended to direct attention to the general method, and the importance of further investigation, rather than to furnish a complete solution of the problem.

Mr. Williams shortly afterward obtained a so-called "Research Fellowship"; but "research" in this case was interpreted as meaning practical work done in a physical laboratory away from Bangor, so the award had the effect of preventing the continuation of original work on this important problem. On the other hand, the necessity of providing, with one assistant, classes in all grades of pure and applied mathematics, and of devoting special attention to the requirements of junior students whose knowledge of the "first four books" and of arithmetic had been neglected at school, left no time for me to carry on the work single-handed. It is only since the comparatively recent abolition of these *infra* university courses that I have been able to give any attention to the subject.

Some criticisms having been raised by the late Captain Ferber, mainly referring to the form in which the conditions of stability were stated, I suggested his developing the work as I had not time to do so. His results were published in the Revue d'Artillerie, October and November, 1905, and include a discussion of lateral as well as of longitudinal stability.

At the beginning of last year the work of my department was, for some unknown reason, exceptionally light, and I had in Mr. E. H. Harper an assistant well able and willing to collaborate in a much more exhaustive investigation both of longitudinal and lateral stability. About October I received a formal letter of inquiry from the Government Committee, in an envelope which I at first took for an income-tax application, and in reply stated that what I wanted was a small grant to enable me to devote my whole time to this work. I received a reply that the committee "regretted," etc., but that "very great interest was taken" in the work. The main difficulties of the subject have, however, now been practically cleared up, though a long time must elapse before a detailed written account is ready for publication. Had any prizes been offered in England for which such an investigation would be eligible, the delay might have been avoided or shortened.

Reference must be made also to Mr. Lanchester's remarkable investigations, published in his "Aerodonetics," and to the appearance of a German translation of the preceding volume, "Aerodynamics," shortly after its publication in English.

It is here proposed to give a general idea of the peculiarities of aeroplane stability as deduced from my work, and a comparison with Ferber's and Lanchester's methods; though with regard to the latter it is rather difficult for any critic to be sure of not misjudging the author's intended meaning.

It is necessary that the distinction between equilibrium and stability should be kept in mind. An aeroplane is in *equilibrium* when traveling at a uniform rate in a straight line, or, again, when being steered round a horizontal arc of a circle. A badly balanced aeroplane would not be able to travel in a straight line. The mathematics of aeroplane equilibrium is probably very imperfectly understood by many persons interested in aviation, but it is comparatively simple, while the theory of stability is of necessity much more difficult.

It is necessary for stability that if the aeroplane is not in equilibrium and moving uniformly it shall tend toward a condition of equilibrium. At the same time, it may commence to oscillate, describing an undulating path, and if the oscillations increase in amplitude the motion will be unstable. It is necessary for stability that an oscillatory motion shall have a positive modulus of decay or co-efficient of subsidence, and the calculation of this is an important feature of the investigation. A slight reference to this question of rolling is given by Chatley on p. 99 of "The Problem of Flight," but he seems to have overlooked the fact that this damping may be, and often is, negative in the case of unstable aeroplanes.

At the present time it is certain that aviators rely on their own exertions for controlling machines that are unstable, or at least deficient in stability, and they even allege that, owing to the danger of sudden gusts of wind, automatic stability is of little importance. Moreover, even in the early experiments of Pilcher, it was found that a glider with too V-shaped wings, or with the center of gravity too low down, is apt to pitch dangerously in the same way that increasing the metacentric height of a ship while increasing its "statical" stability causes it to pitch dangerously. It thus becomes important to consider what is the effect of a sudden change of wind velocity on an aerodrome. If the aerodrome was previously in equilibrium it will cease to be so, but will tend to assume a motion which will bring it into the new state of equilibrium consistent with the altered circumstances, provided that this new motion is stable. Thus an aerodrome of which every steady motion is stable within given limitations will constantly tend to right itself if those limitations are not exceeded. Excessive pitching or rolling results from a short period of oscillation combined with a modulus of decay which is either negative (giving instability) or of insufficient magnitude to produce the necessary damping.

The new work depends very largely on the property that for a system of narrow aeroplanes inclined at small angles to the line of flight approximate methods may be used, greatly simplifying the algebra, and enabling the various oscillations to be separated and their moduli of decay to be calculated approximately. Of the six equations of motion as applied to the small oscillations of a symmetrical aerodrome, three determine oscillations of symmetric or longitudinal stability. The other three determine asymmetric or skew symmetric stability. The three equations in each set are mutually interdependent, but independent of the other three, thus accounting for the fact that Lanchester found it impossible to separate "lateral" and "directional" stability. Failing any better terminology, I have provisionally adopted the term "asymmetric" stability.

Of the two, symmetric stability presents by far the simpler problem. For the systems above mentioned there are two symmetric oscillations, one of long and one of short period. The short-period oscillation consists mainly of an oscillatory motion of the center of gravity perpendicular to the line of flight (i. e., a vertical oscillation if the aerodrome is moving horizontally), combined with a rotatory oscillation about the center of gravity. To a first approximation it produces no fluctuations in the velocity in the line of flight, and is unaffected by head resistance or fluctuations in the propeller thrust, provided the latter passes through the center of gravity of the aerodrome, as has been assumed in many of our calculations. The condition of stability depends only on the areas and positions of the aeroplanes relative to the center of gravity, and is independent of the inclinations or angles of attack of the planes, the oscillations remaining finite when the planes are parallel. This condition of stability is generally satisfied in any arrangement which satisfies the other conditions of stability. It must not be overlooked though it is very unlikely to give trouble. The corresponding trajectory or curve of oscillation is independent of the velocity, the actual time rates of oscillation and decay being proportioned to the velocity.

In the slow oscillations the variations of velocity in the line of flight are a predominating feature. The trajectory is wave-like, the crests of the waves being more pointed than the troughs, and the descending parts steeper than the ascending ones. This is evidently the type of oscillation studied by Mr. Lanchester. One condition of stability is that the front plane (or planes) must be inclined at a greater angle than the rear ones. The second condition depends on the type of machine.

The terms "monoplane" and "biplane," as usually defined, refer to the question of whether a machine has not or has superposed planes. According, however, to a property which I call the principle of independence of height, this distinction does not affect stability to any appreciable extent. The important point is whether the weight is sustained partly by the front and partly by the rear planes, as in certain Voisin machines, or is wholly supported by the front planes, the rear ones acting merely as a tail in the neutral position. For a monoplane with neutral tail the condition of stability takes the form given by Lanchester, when the necessary substitutions have been made by making use of the condition of equilibrium. The reason why Lanchester's method leads to a correct result is to be sought in considerations of the peculiar nature of the oscillations, and in especial in the relative smallness of their modulus of decay. For a machine of the Voisin type, with sustaining surfaces arranged tandem, the condition of stability is nearly as simple, and certain modifications are sufficient to cover the case when the propeller thrust does not pass through the center of gravity provided that this thrust is constant.

A very convenient plan in such cases is to suppose the actual machine replaced by an equivalent monoplane, with neutral tail, although if the inclinations of the planes be varied for vertical steering the equivalent monoplane will be changed.

The most remarkable result, however—and Mr.

* Nature.

Harper was the first to point this out to me—is the important effect on stability of the direction of motion in the vertical plane. Longitudinal stability falls off rapidly when the aeroplane begins to rise, even if other things are constant. A monoplane would, under theoretical conditions, become unstable when ascending at an angle to the horizon of less than twice the angle of attack (or indication of the main plane to the line of flight).

The effect of head resistance is to increase the stability, and a further increase occurs if the thrust of the propeller, instead of being constant, decreases when the velocity increases. By the use of three planes instead of two, an additional increase of stability can be obtained. On the other hand, if the aeroplane be gliding downwards the longitudinal stability is greater than in horizontal flight.

I think the above conclusions indicate a source of danger which may possibly have led to mishaps when aeroplanes have risen too rapidly in the air.

Capt. Ferber's investigations, on the other hand, refer mainly to the stability of a single aeroplane as dependent on fluctuations in the position of the center of pressure consequent on variations of the angle of attack. He assumes Joessel's formula, introducing two arbitrary constants in place of the numerical co-efficients. The difficulty I have several times pointed out is that, if a plane is turning over, its rotational motion may affect the position of the center of pressure, as well as possibly the resultant thrust, and no experimental information is apparently available on this point. For this reason the use of narrow aeroplanes is to be recommended, stability being the other. Moreover, the theory of narrow aeroplanes gliding at small angles affords the simplest introduction to a general study of aeroplane stability, just as geometrical optics in which aberration is neglected affords an introduction to a general study of lens construction. It is to be remembered that both the symmetrical and asymmetrical oscillations are determined by equations of the fourth degree, each in the form of a determinant of the third order containing the dynamical constants and resistance co-efficients, and when this determinant has been expanded, four conditions of stability have to be satisfied, one being Routh's discriminant $BCD - AD^2 - EB^2$ shall be positive. Fortunately, for purposes of approximation, $CD - EB$ may be substituted for the last in many of the systems occurring in aviation. It will thus be seen that stability is a very complicated problem, and that approximate methods are essential.

Asymmetric stability is far more difficult of investigation than symmetric. It is necessary to take account of the separate effects of straight or horizontal aeroplanes, vertical fins, and bent-up or V-shaped planes. The late Capt. Ferber's solution is based on the substitution for the actual planes of their projections on three co-ordinate planes (p. 46 of his paper). Unfortunately, even assuming the sine law of resistance, this substitution does not seem to give even the correct first approximation which is all the author claims. In particular, if the aerodrome is rotated about any axis in its plane of symmetry, couples are set up on the main aeroplane which have an important effect on the stability, but are apparently not included in his scheme. The final result is a biquadratic with one root equal to zero, and Capt. Ferber regards an aeroplane as stable when it describes a helix; whereas such an arrangement should really be regarded as lacking in stability. The couples in question are taken account of by Lanchester, who uses what he calls "aerodynamic and aerodromic radii" to represent their effects. For a narrow aeroplane gliding at a small angle, the effect depends on the moment of inertia of the area of the plane about the vertical plane of symmetry. A horizontal tail of negligible lateral dimensions does not affect the asymmetric stability.

To secure stability, recourse must be had to vertical fins, or to bent-up aeroplanes or aerofoils. The effect of vertical fins (neglecting "wash") depends on their areas, and the first and second moments of these about the axes, and in studying them it is necessary to have recourse to the "principle of parallel axes." The sections in "Lanchester" on "fin resolution" practically embody this principle, but are a little difficult to follow; they suggest the path taken by an explorer who had not a compass to guide him to the mathematically direct road in the form of the principle in question. His conditions of stability seem reasonable deductions from the hypotheses he makes, but the conclusions must not be regarded as final. Both the necessary and the sufficient conditions of stability are really far more complicated, and it is highly improbable that the problem could have been carried much further without the elaborate use of analysis which I have found necessary, and the assistance of an independent calculator, which Mr. Harper has kindly provided. The only way of proceeding was to calculate the co-efficients in the biquadratic for particular arrangements of fins and planes, starting with the simpler ones, and passing to more complicated ones when one has become thoroughly familiar with the different terms and their meanings.

For an aeroplane with one vertical fin only, the conditions of asymmetric stability require that the center of pressure of the fin should be slightly in front of the center of gravity of the machine, while at the same time it should be at a height above the center of gravity large compared with its distance in front. Two of the conditions of stability are difficult to reconcile with the conditions of equilibrium, the difficulty increasing as the velocity increases and the angle of attack diminishes; moreover, they are inconsistent unless a certain relation holds between the radii of gyration of the machine and of the main supporting surface. It is doubtful whether this condition would be consistent with practical requirements.

The failure of practical aviators to obtain automatic stability may be due in no small measure to the number of conditions that have to be satisfied. A vertical fin in front might satisfy one condition of stability, and introduce instability through another condition, while a similar fin at the back might satisfy the second condition and introduce instability through the first. In either case the impression produced would be that the device secured automatic stability, but that such stability was a hindrance rather than a help, the correct interpretation being that the conditions of stability had not been sufficiently investigated. By abolishing the fins the aviator would obtain a machine with defective stability, i. e., with one or more roots of the biquadratic vanishing, and would find it easier to maintain his balance by artificial control than in the previous unstable arrangement.

Of arrangements with two fins, the cases have been considered where both fins are at the level of the center of gravity, where one is above, and where both are above. The conditions of stability assume various forms, but there is one arrangement which appears to possess an exceptionally wide range of stability, and I have made provisional application for a patent in this connection.

A machine such as the Voisin type, with two planes of considerable span at different angles of attack, is more stable than one with a single sustaining system, and the difference is equivalent to a variation in the arrangement of the fins which is easily calculated.

The asymmetric oscillations of an aerodrome do not separate into two kinds, of long and short period, like the symmetric ones. As a general rule the biquadratic has one root determined approximately by the first two terms representing a quick subsidence, one root determined by the last two representing a slow subsidence, and a pair of roots determined by the middle terms representing a damped oscillation.

The inclination of the flight-path to the horizon has a considerable influence on the asymmetric stability. In several instances we found that instability occurs when an aerodrome is descending at an angle to the horizon the tangent of which is double that of the angle of attack of the main planes. Other arrangements become unstable when rising at more than a certain angle; in the best arrangement referred to above the stability is practically independent of the inclination. As the symmetric and asymmetric oscillations of an aeroplane are independent, it is important that it should preserve its asymmetric stability even when it is not in longitudinal equilibrium. The dependence of stability on inclination affords a very simple and likely explanation of certain "vagaries" described on pp. 342, 343 of Lanchester's "Aerodonetics"; account would, however, have to be taken of accelerations of the center of mass in an exact comparison of theory with observation.

Bent-up or V-shaped wings lead to much more difficult analysis, and it appears that their effect is not exactly equivalent to any combination of vertical fins except in certain cases. A pair of "stabilisers" or small planes, which may be fixed at the extremities of the main aeroplanes at an angle of, say 45 deg., is equivalent to a single raised vertical fin if the planes of the stabilizers are parallel to the line of flight.

Mr. Harper has worked out the asymmetric stability of the Antoinette type with a single pair of bent-up supporting surfaces. The conditions of stability are satisfiable by furnishing the machine with a tail of suitable length, or by raising the dihedral angle of the V-shaped wings above the center of gravity.

I should like to direct attention to the importance of eliminating the personal element in experimental tests of aeroplane stability, by the use of models. The possibility of long-distance flights by skilled aviators having been demonstrated, there is not so much point in repeating these verifications as in extending our knowledge in other directions, and finding how far the element of skill can be dispensed with by effecting improvements in aeroplane design.

The stability of dirigibles opens up another field of study, on which we hope to do something.

Owing to the attention now given to aeroplane construction, it appeared desirable to give, in the present article, an advance account of investigations which may not be ready for publication in extenso for some time to come.

The Aeronautical Journal for January, 1910, includes a short abstract, illustrated by badly executed diagrams, and containing numerous uncorrected printers' errors, in which the symmetric stability of a single-surfaced aerodrome without tail is made to depend on a cubic instead of a biquadratic equation. This result is obtained by the very doubtful method of "assuming V to be constant for a short period," that is, neglecting fluctuations in horizontal velocity. Owing to this assumption the conclusions reached may perhaps represent the conditions that the machine may be stable with reference to the shorter oscillations, but not with respect to the longer ones, and the inference that a machine can be much more stable at moderate velocities than is generally supposed must not be regarded as conclusive.

THE REMARKABLE CONDUCT OF A DROP OF MERCURY.

It has been known a passably long time already that lower organisms that are capable of independent movement, such as amœbæ, infusoria, bacteria and others, are attracted by certain chemical substances. For instance, fill a capillary tube with a weak solution of chlorate of potash or of peptone and put into it a drop of mercury in which bacteria are moving; after a few seconds these will be seen hastening to the mouth of the tube where they will all have assembled. The amœbæ and the naked little masses of jelly (plasmodia) of the myxomycetes (mucous fungi) creep in their peculiar way by stretching forth their arms or feelers toward the stimulant. This faculty of these organisms of being attracted by certain substances is called chemotaxis. Chemotactic susceptibility is evidently an advantage for the creatures in question as it leads them to good nourishment and keeps them near it.

And very recently an eminent physiologist made the discovery that a drop of mercury can make very similar movements. The starting point of his observations was afforded by the experiment made by Paalzov in 1858. The latter put a drop of mercury in a little flat vessel; over this drop he poured diluted sulphuric acid and then laid a small crystal of bichromate of potash immediately beside the mercury. The result was a periodical change in the shape of the drop of mercury, which alternately approached the crystal, while flattening itself in front, and receded from it. This occurrence was provoked by the fact that the bichromate of potash, aided by the presence of the acid, oxidized that portion of the surface of the drop of mercury turned toward it and thus diminished the tension of the surface of that side of the drop. As soon as the peroxide of mercury, which had been produced, dissolved in the sulphuric acid the surface of the mercury became metallic again and its tension increased. In the first instance the mercury flowed toward the crystal, in the second it sprang back.

In a recent number of the Archiv fuer die Gesammte Physiologie the professor explains his application of this experiment. Through appropriate manipulation he imparted to a drop of mercury the faculty of real locomotion. One of the most successful forms of his experiment was this: he put a drop of mercury in a suitable glass dish of which the bottom was perfectly level; then he poured in a sufficient quantity of diluted nitric acid and laid a little piece of bichromate of potash at a distance of several centimeters from the drop of mercury on the bottom of the dish. The yellow solution of the crystal began to spread itself in a circle and as soon as it reached the drop of mercury the latter with a curt tremor began to recede and then dashed straight to the crystal which it reached in a few seconds. In the liveliest manner it repeated the twitching movements already described. If, in consequence, the crystal moved away in any direction the drop pursued it, receded and approached, again and again, with a movement of mingled leap and glide, which stretching forth long tentacles and quickly drawing them back again. This lively play leads an observer to think that these movements are those of a living organism. They last until the crystal is consumed or the drop has accidentally moved too far away from it. These remarkable phenomena may be considered as adequate support of the view held by the botanist Barthold, the physicist Quincke, and the physiologist Verworn, that the amœboidal and related movements are the result of changes in the tension of the surface of the living substance. Obviously, though, there are still other conditions which can vary largely the movements of the living prototype.

SCIENCE NOTES.

The great heights which balloon artillery must be able to reach necessitate an extension of the range tables, and so photographic reproductions of the flight of shot, although affording little information as to velocity, are of particular interest. Two photographs are published in the Phys. Zeitschr. in connection with an article by F. Neesen, the first showing the path of a shot which emits smoke, the second showing the end of the flight of five luminous shot from a mountain gun. The latter photograph was, of course, taken in twilight; the path is much more clearly defined in this plan than with the smoke shot, and there is the additional advantage that the paths of several shot can be shown on the same plate; also information is given with respect to the direction of the axis of the shot, and as to its oscillation.

During the opposition of 1909, J. C. Sola made some observations of Mars at Fabra Observatory, which he discusses in Comptes Rendus. A double equatorial of 0.38-meter aperture was used, with a magnification of 450, and sometimes 550. The satellites Deimos and Phobos could be followed. The chief conclusions derived are: (1) The main topographical outlines remain constant (Sola has observed Mars since 1890). The small details vary, doubtless owing to cloud, and, perhaps, vegetation. (2) The tonality is very changeable in the dark regions but practically invariable in the light regions. From this it is inferred that the clouds are of similar color to the "lands" or that the "lands" are always covered with snow. In general, the details of the south hemisphere grew darker and more plentiful with the diminution of the southern snows, that is with increase of temperature. Syrtis Major, faint and scarcely perceptible in August, was dark and well defined in October. The canal Bathys was seen during this apparition.

The problem of the rotation of Mercury is one of considerable astronomical importance. First made the subject of more or less violent discussion by Schiaparelli, the problem recurs almost annually. One of the more recent discussions is to be found in the Comptes Rendus by R. Jarry-Desloges. The paper contains reproductions of sixteen drawings of Mercury, viz., eight by G. Fournier, and eight by V. Fournier, from which the author infers that the rotation-period is long, probably equal to the planet's time of revolution. The drawings were made on the Massegros (altitude 900 meters) with a refractor 0.29 meter aperture, on evenings September 15-20, 1909, about half the planet's disk being illuminated. They show a very few dark shaded streaks, two or three branching from the central region, one parallel to the southwest limb, and one cutting across the north cusp. A dark dot is shown on some of the drawings in the southeast, one observer placing it at the intersection of two streaks. A dark region is indicated near the equator. The south horn is sometimes shown rounded or shaded. There is fair approach to agreement between the drawings of the two observers, considering that the objects delineated are at the limits of visibility. Jarry-Desloges remarks that in the short moments when the images were less unsteady than usual, the dark regions on Mercury appeared as dark as the average tint of those on Mars; and the difficulty in rendering details is mainly due to the bad quality of image peculiar to this class of study.

Beating wing flight is considered in the Royal Society Proceedings by M. F. Fitzgerald. The resistance opposed to the acceleration of a body immersed in a liquid may be represented by a virtual addition to its mass, without any corresponding addition to its weight. A wing flapping in air may shed, or escape from the virtual added mass at will, much as the paddle of a Canadian canoe can slip out of the disturbance it has created during a stroke. Hence a heavy body or engine attached to a wing should be capable of supporting itself and the wing in the air in a succession of leaps. For if the whole be supposed initially falling, a sufficiently energetic pull given by the engine, drawing the wing down and itself up, could convert the fall of their common center of gravity into a rise, if there were a weightless but massive body temporarily attached to the wing. If, when this rise had been started, the mass were shed or escaped from, there would be an interval in which the center of gravity of the whole would continue to rise, during which the wing and body could be replaced in their original relative positions by mutual action between them, wholly internal to their system, free except for gravity. If at the end of the rise added mass were again picked up, the same might fall to its initial level and velocity. The process thus becomes cyclic, and may be indefinitely repeated. The author calculates the power required to carry 1 pound in the case of a "wing" in the shape of a flat circular disk 12 feet in diameter, for various values of the ratio of the additional virtual mass to the weight. The weight carried per horse-power is a linear function of A, and amounts to 34 pounds per horse-power, when $A = 1$. It varies directly as the rate of flapping.

ELECTRICAL NOTES.

According to a statement issued by the Siemens-Schuckert Company of Berlin, the felling of trees by means of wires heated by electric currents, which has been described in various newspapers, cannot be accomplished in a practical and economical manner, for the following reasons: The wire, to cut effectively, must be very tightly stretched and it is therefore very liable to rupture, in consequence of its high temperature. The red-hot wire carbonizes the wood, and the charcoal, if allowed to accumulate, protects the interior parts from the heat of the wire. In order to remove the charcoal, the wire must be roughened and moved to and fro lengthwise, so that the operation is still a sort of sawing, and the motion and roughening increase the liability to rupture.

The effect produced on the form of an alternating current by introducing a selenium cell into the circuit has been investigated by A. Pochettino and described in Accad. Lincei. Atti. The results are given, together with curves, for different types of cell. In nearly all cases the wave-form is altered, the area inclosed by one half of the curve being diminished with respect to the other, and the ratio between the difference in areas contained by the two half-waves, and the greater of these, which Pochettino denotes by R, is a measure of the rectifying power of the cell. The value of R varies from 0.06 for a cell with Al electrodes to 0.30 for a Kohl cell of old construction, and in general diminishes with increase in voltage, or decrease in resistance, external influences, such as heat or light, which reduce the resistance, also diminishing R.

When an electrolytic cell of the aluminium type, containing a solution of a borate as electrolyte, has been in continuous operation for some time, an objectionable deposit is liable to be formed on the plate. In an invention recently patented in this country and in England this deposit is in great part suppressed, and its injurious effects are reduced, by the addition of tartrates to the electrolyte; this also improves the character of the film, in cutting down the leakage current and in raising its critical or breakdown voltage, besides reducing the resistance of the electrolyte. The solution may be made by mixing 6 parts of boric acid and 1 part of tartaric acid and then approximately neutralizing the mixture with ammonia or other alkali; or a tetraborate may be used instead of boric acid. In some cases, it is preferred to add also about 10 per cent of glycerin, and, in a modification of the invention, the acidity thus produced is neutralized or partly neutralized by again adding alkali.

The city of Naples is taking measures to secure a large amount of current for light and power purposes from a hydraulic plant which is to be located at 55 miles distance at the sources of the Volturno stream. These sources are received in a number of reservoirs and the hydraulic work will comprise a 2-mile flume and a set of three penstocks in order to supply the turbines. The station will be located on the Volturno stream, and it is thought that it will furnish 10,000 or 15,000 horse-power. Alternators working on the three-phase 5,000-volt system will be used, and a set of transformers will raise the voltage to 45,000 for use on the overhead power line leading to Naples. In the city the line is received in a transformer station in which the tension is lowered so as to feed the city mains. These are to be underground within the city limits, but it is also intended to run several overhead lines for use in the suburbs. In connection with the transformer station there will be erected a steam plant which serves as a standby and will be able to give about 6,000 horse-power, so that the city will dispose of a total of 16,000 horse-power for the new plant. It is also intended to use a second fall on the Volturno stream which lies below the preceding, so that the total amount will be increased to 20,000 horse-power.

TRADE NOTES AND FORMULÆ.

Modeling Wax (for Making Fruits, Leaves, Etc.)—10 parts white wax, 1.5 parts mutton fat, 1.5 parts dark rosin melted together, add 1 part of cinnabar (vermilion), stir, take from the fire, pour into rod-shaped tin molds where it must be stirred until it sets. (Use also other non-poisonous colors.)

Embossing Wax.—Modeling wax for rough work.—A mixture of 3 parts turpentine, 5 parts wax, and some olive oil or hog's fat; to make it non-transparent add red lead or cinnabar. For finer work, 100 parts pure white wax, 132 parts pure hog's lard, 132 parts black pitch, 66 parts cinnabar or vermilion.

Preservation of Antique Wall Paintings.—Impregnate them with a solution of paraffine. In the case of fresco and stereochromic paintings, defective spots are to be gradually saturated with silico-fluoric acid and water glass, then painted with colors (which may be applied by means of water glass) and then treated with a fixative and finally impregnated with paraffine.

Preservation of Leather.—Melt 3 parts of common washing soap with 1 part of palm oil and add to it 4 parts of ammonia soap (obtained by saturating oil soap with carbonate of ammonia) and 1.75 parts of a tannin solution, containing 9 to 16 parts of tannic acid dissolved in 4 parts of water. The unguent thus obtained can be kept for a long time in well-corked stone vessels and should be used only in such quantity as the leather will conveniently take up.

Substitute for Bristles and Whalebone.—The fibers of a suitable plant (piasava, alpha grass, etc.) are joined together by means of a suitable adhesive. For the latter purpose use solution of silicate of soda alone or mixed with heavy spar, felspar or chalk, or any kind of gum, glue, shellac, etc. The cemented mass is cut into strips and after drying is given a coating of celluloid, glue, etc. To make the mass water proof, it should be given a coating of rubber solution, copal, or similar resins. Finally, the composition is covered with metal foil, silk, cotton, linen, etc.

Production of Carmine.—1. 125 parts of finely pulverized cochineal is boiled with 5,000 parts of water for 15 minutes, and after adding 30 parts powdered alum, allowed to stand hot until the fluid is fairly clear, then quickly strain it through a linen cloth. After 24 hours, pour it off from the separated carmine (40 to 50 parts), and in another 3 days it will again yield some carmine, but of inferior quality. 2. 500 parts of cochineal powder is boiled for 15 minutes with 15,000 parts of water, 30 parts pulverized cream of tartar added and boiled for ten minutes; 15 parts of powdered alum boiled for 2 minutes. Allow to settle hot, expose the clear fluid in shallow glass dishes in a very light place and the carmine will be deposited as a fiery red powder.

Cigar Perfuming Box.—Melilot clover 200 parts, tonka beans 50 parts, oil of roses 1 drop (clover is cut very evenly fine, passed through a very fine sieve and thereby freed from dust). The tonka beans are first cut into fine strips, like nudels, then across into small cubes and mixed with the clover. Finally, the genuine rose oil is dissolved in alcohol perfectly free from fusel, and the above mixture sprayed as evenly as possible with the fluid. Other perfumes may also be sprayed on the clover-tonk bean mixture: 1. Valerian tincture 200 parts (obtained by allowing 1 part of valerian root to stand 8 days in 8 parts of fine alcohol, frequently shaking and finally pressing it out); tonka bean tincture 200 parts (20 tonka beans in 180 parts of fine alcohol); benzoin tincture (50 parts gum benzoin dissolved, with shaking, in 1,000 parts of alcohol and filtered). 2. Valerianic acid 10 parts, benzoin tincture 100 parts, alcohol 1,900 parts, acetic ether 40 drops, butyric ether 10 drops. 3. Vanilla tincture 8 parts (1 part vanilla, 5 parts of fine spirit), tincture of valerian 15 parts, butyric ether 15 parts, spirits of niter 4 parts, alcohol free from fusel oil 1,000 parts.

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